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A Case of Mal-formation, with an Engraving.

To the EDITORS of the MEDICAL and PHYSICAL JOURNAL.

Gentlemen,

THAT peculiarity in the fœtus, which consists in its deprivation of brain, is not very uncommon; I met with an instance of it eight years ago, and have seen others in different anatomical collections. The deficiency in the subject I am about to describe, is extended, to the medulla spinalis; and as it is new to me, and has given rise to some reflections, which may not be altogether useless, I have taken the liberty to transmit them for insertion in your Journal.

The mother was delivered at Bolton, by Mr. Barlow, to whom I am indebted for the history of the case, and for the fœtus itself. The only material circumstance in the former, was, the woman's positive assertion that she went two months over her time, which, however, I am inclined to doubt, and would rather suppose, that she had committed an error in her reckoning. In the cases most analogous, the birth has been usually premature, generally at the seventh month, and it happened so in my former case.

In this child, the upper part of the cranium is entirely wanting; and there remains only a thin plate of bone, covered with a doubling of membrane, in place of the cervical and the greater part of the dorsal vertebræ. This fold contained no medulla, though it exhibited, on being slit open, some slender fibrils, which might be construed into nerves; I should compare it to the proper coverings of the medulla spinalis, of a thinner texture. Lower down, a displaced portion of vertebræ is shewn, which was hollow, but contained no medulla; the rest of the spine consisted of a solid column of bone, without any spinous processes. The child had, besides, a slight inversion of the feet, and a hare-lip on the right side; in other respects, it was full grown, and the colour o the skin was natural.

Sketch of a Monstrous Fœtus.

Printed for R. Phillips No. 71 St. Paul's Church Yard.
Hemsley, sc.

There did not appear to be any deviation from the common ſtructure and arrangement in the viſcera of the thorax and abdomen: the heart, lungs, and thymus, occupying the former cavity, in their proper order; and the ſtomach, liver, ſpleen, kidnies, great and ſmall inteſtines, &c. the latter. The larger inteſtines were alſo diſtended with meconium.

In the cheſt too, I traced the phrenic nerves, deſcending to the diaphragm in their uſual courſe; and in the neck, the par vagum, with its ganglia and intercoſtal, lying between the carotid artery and internal jugular vein. Though the eyes were outwardly well formed, I could not find by diſſection any optic nerve.

The nerves in the upper and lower extremities were, nevertheleſs, perfect; for I traced them in the arm and in the thigh, and in neither did I obſerve any difference in their number, ſize, colour, or diſtribution.

This fœtus was ſtill born, which, if I miſtake not, has always been the caſe when the brain has been wanting. However, the mother was not ſenſible, during pregnancy, of any difference from what ſhe had been formerly accuſtomed to, either in her own feelings or in the motion of the child; and ſhe had had many children. The birth was marked by no particular occurrence; it would probably be facilitated by the reduced bulk of the head.

In comparing the defective ſtructure of this child with the aſcertained uſes in others of thoſe parts of which it was deprived, I have been led to conclude, that nervous influence is not at all neceſſary to the growth of the fœtus in utero. At an early period after conception, it is highly probable, that the augmentation of the fœtus is maintained by the circulating ſyſtem alone; and as it is ſelf-evident from this caſe, that it can go on at a later, without either brain or ſpinal marrow, the nerves muſt grow like the other component parts of the body, and perfectly diſtinct from any other influence than that of the circulation.

It is proved by experiment, that when the ſpinal marrow, or principal nerves of a limb are divided, the parts below are immediately deprived of their ſenſibility, and become torpid; hence, we may reaſonably infer, that no peculiar property is reſident in the nerves themſelves. Aſſuming then, that the nerves ſerve merely to convey the influence of the brain and medulla ſpinalis, it is obvious, that when deprived of theſe ſources, they can impart none. Thus, it is evident, that although this fœtus had attained the full ſize, and its motions were not perceptibly different from another, yet, having no ſenſorium, it could poſſeſs no ſenſation.

Throughout

Throughout all Nature we obferve the wifdom of Providence, in adapting the ftructure of every animal to its peculiar mode of exiftence. In the fœtus, we note feveral contrivances for the uterine ftate, which become unneceffary foon after birth; as the foramen ovale, the funis umbilicalis, and the ductus arteriofus; the thymus too may be numbered, though its ufe be at prefent unknown: others, as the lungs, then lie dormant, and are called into activity by its change of condition. But, to beftow their proper functions on the nerves, would then be at leaft a work of supererogation, as there is no object to which the impulfe derived from fenfation could be deftined. On the contrary, fenfibility would expofe it to hazards, which Nature has been fedulous to avert, by depriving the funis of nerves, and by furrounding it with the liquor amnii.

Mr. Barlow informs me, that he has repeatedly tried the experiment upon a prefenting upper or lower extremity, and that the refult has always confirmed my opinion.

As far as I have been able to determine, fenfation is coeval with refpiration; for when, after birth, refpiration has been delayed, and during the pulfation in the funis no appearance of fenfation has arifen till the child began to breathe, the functions of the lungs, and of the nervous fyftem, were then roufed into activity in the fame moment of time.

I purpofely avoid any further difcuffion, though the fubject is pregnant with much curious matter. My object now has been to prove,

1. That nervous influence is not at all neceffary to the growth of the fœtus in utero; and,

2. That the fœtus in its uterine ftate does not poffefs fenfation.

The figures will require but little explanation. In No. 1, the dark colour, as it appeared in the fubject, in the direction of the brain and fpine, fhews the deficiences in thofe parts; and the probe, the hollow portion of vertebræ. A little lower down, the light reprefents the protuberance of the fpine, the folid ftructure of which is delineated in figure, No. 2; and under the left arm, is given a portion of inteftine, diftended with meconium. A cloth was thrown over the legs, becaufe it was thought unneceffary to reprefent them. The natural and healthy appearance of the fkin is preferved, and the maturity of growth well difplayed. The nerves have not been delineated, as they contained nothing remarkable, either in their ftructure or diftribution.

Manchefter, July 29, 1800. W. SIMMONS.

N. B. The Drawing is made of the natural fize.

To the Editors of the Medical and Phyſical Journal.

GENTLEMEN,

AS the firſt object of your valuable Publication is to diſſeminate the knowledge of uſeful diſcoveries, ſo, I preſume, the next, in order as in utility, will be to prevent the propagation of error; and if, in execution of the firſt part of your plan, you ſhould accidentally promulgate any thing, under the idea of its being an uſeful diſcovery, though, in fact, it was previouſly well known, and proved by experience, to be inadequate to what it profeſſes, I apprehend you will not be unwilling to admit ſuch obſervations as may tend to reduce it to its juſt value in the eſtimation of your readers.

The conſideration of theſe circumſtances, which, I preſume, regulate the conduct of your Publication, has induced me to trouble you with the following obſervations on a Communication from Mr. R. WATT, in your laſt Number, which he calls, Deſcription of a New Machine for curing diſtorted Limbs. My intention is to ſhow, firſt, that it is not a new diſcovery; and, ſecondly, that though it may be uſeful on ſome occaſions, it is abſolutely incompetent to effect all that he ſo unreſervedly attributes to it.

In doing this, I have no intention to impeach Mr. Watt's veracity, or the purity of thoſe motives which, he ſays, have induced him to communicate his diſcovery to you; for it may be granted, that he has known nothing more on the ſubject than what has emanated from his own mind; it may be granted too, that he has cured by this inſtrument, ſome deformity that he calls varus, or valgus; and yet, all that I mean to advance in abatement of his claim to the originality or utility of his invention, may be ſtrictly true.

When profeſſional men, in retired ſituations, are induced to turn their minds to particular ſubjects, without previous information, they conclude, and juſtly too, that whatever ariſes in their minds, is *their own invention*; — but, when they introduce their diſcoveries to the world at large, they will often find, and perhaps not without chagrin, that what was new to them, had been previouſly long and well known to many; that, what in their limitted experience ſeemed to be advantageous, had been found ineffectual in the hands of many others; and, they will gain this advantage by this diſcovery, viz. to be more guarded in their aſſertions reſpecting their own imaginary inventions, and more cautious in aſſigning unworthy motives to thoſe who may not act exactly as they have thought proper

proper to do. This I believe to be Mr. Watt's ſituation, for the following reaſons:

Firſt, When I was in Dublin, in the year 1796, a gentleman employed me to cure his child's diſtorted foot. Upon enquiring what, if any thing, had been done previous to my ſeeing the caſe, I was told the late Mr. Deaſe had been conſulted, and he adviſed the parents to ſend for a pair of the well-known Edinburgh boots: he deſcribed theſe as being intended to preſs and compreſs the feet till they were reduced to the natural form. I had no communication with Mr. Deaſe on the ſubject; but as he deſcribed the inſtrument as being well known in Edinburgh, and attributed the ſame powers and mode of operation to it as Mr. Watt does to his *invention*, I have ſome reaſon to conclude they are not very diſſimilar.

Secondly, Dr. Aitkin, in his Eſſays on Fractures, &c. deſcribes an inſtrument for the ſame purpoſe, and which is to act in the ſame manner upon the diſtorted feet of children; he gives a plate of this inſtrument, which certainly reſembles that of Mr. Watt's in part. In deſcribing this plate, he ſays, the machine much reſembles the boot of Hildanus. Here is both ancient and modern information, which have moſt unaccountably eſcaped that ſpirit of reſearch, which induced Mr. Watt to turn over "every volume of ſurgery he could lay his hand on, without finding any thing to his purpoſe."

Thirdly, It is at leaſt fifty years ſince a ſhoe-maker, whoſe name was Holmes, made known, as his invention, a method of curing the diſtorted feet of children: It conſiſted in binding them firmly, and then inſerting them in a pair of boots or ſhoes, that are connected together by means of a ratchet and ſpring. His intention was, like that of Mr. Watt, to preſs upon the convex ſide, and then twiſt and turn the feet out; and we ſee the means he uſed are the ſame. As this man practiſed his art for the reſt of his life, and a perſon is ſtill living who continues to do ſo, their long experience may prove more than Mr. Watt's; it is well known that they have *imperfectly cured* ſome very ſlight caſes, but they never were able to cure completely any one important caſe of diſtortion.

Having ſaid thus much on the *originality* of Mr. Watt's invention, it remains to conſider its *efficacy*; for, after all that can be ſaid, it is of little conſequence whether a man makes a diſcovery, borrows the deſign of another, without leave or acknowledgment, revives an obſolete or gives publicity to an obſcure invention, provided that ſociety at large is benefited by his communication; Mr. Watt ſays the individual is ſure of being rewarded as he deſerves. It is to be wiſhed that the terms varus and valgus, as applied to theſe deformities, were

banіſhed

Throughout all Nature we observe the wisdom of Providence, in adapting the structure of every animal to its peculiar mode of existence. In the fœtus, we note several contrivances for the uterine state, which become unnecessary soon after birth; as the foramen ovale, the funis umbilicalis, and the ductus arteriosus; the thymus too may be numbered, though its use be at present unknown: others, as the lungs, then lie dormant, and are called into activity by its change of condition. But, to bestow their proper functions on the nerves, would then be at least a work of supererogation, as there is no object to which the impulse derived from sensation could be destined. On the contrary, sensibility would expose it to hazards, which Nature has been sedulous to avert, by depriving the funis of nerves, and by surrounding it with the liquor amnii.

Mr. Barlow informs me, that he has repeatedly tried the experiment upon a presenting upper or lower extremity, and that the result has always confirmed my opinion.

As far as I have been able to determine, sensation is coeval with respiration; for when, after birth, respiration has been delayed, and during the pulsation in the funis no appearance of sensation has arisen till the child began to breathe, the functions of the lungs, and of the nervous system, were then roused into activity in the same moment of time.

I purposely avoid any further discussion, though the subject is pregnant with much curious matter. My object now has been to prove,

1. That nervous influence is not at all necessary to the growth of the fœtus in utero; and,

2. That the fœtus in its uterine state does not possess sensation.

The figures will require but little explanation. In No. 1, the dark colour, as it appeared in the subject, in the direction of the brain and spine, shews the deficiencies in those parts; and the probe, the hollow portion of vertebræ. A little lower down, the light represents the protuberance of the spine, the solid structure of which is delineated in figure, No. 2; and under the left arm, is given a portion of intestine, distended with meconium. A cloth was thrown over the legs, because it was thought unnecessary to represent them. The natural and healthy appearance of the skin is preserved, and the maturity of growth well displayed. The nerves have not been delineated, as they contained nothing remarkable, either in their structure or distribution.

W. SIMMONS.

Manchester, July 29, 1800.

N. B. The Drawing is made of the natural size.

banifhed from our Vocabulary; for as they only defcribe the pofition of the foot, without indicating the ftate of the difeafed parts, and are applied indifcriminately to all, they only ferve to confound inftead of illuftrating the fubject. Even Mr. Watt *seems* to have but very crude ideas of the facts of this difeafe; he fays, "THIS DISORDER (i. e. the two difeafes called varus and valgus) may be occafioned by different caufes; by a morbid ftate of the bones, or contraction of the mufcles; by original mal-conformation, or wrong pofition in the womb. *But from whatever caufe the diftortion may proceed, the reducing the bones to their natural pofition muft be accomplifhed by nearly the fame means; that is, by judicious preffure upon the convex side. In fome* cafes the diforder lies in the ancle joint, while the leg and knee are perfectly natural; in others, *and perhaps the greateft number, it is occafioned by a bending of the bones of the leg,* by which the toes are turned either out or in, according as the bones are bent to the one fide or the other."

I fhould be forry to mifreprefent, though the obtufenefs of my intellects may oblige me to mifunderftand, Mr. Watt's meaning: If I do underftand it, it is this; that when the firft cafe was put under his care *he had never feen fuch a cafe treated before*; he immediately began to turn over every volume of Surgery he could lay his hands on; but finding nothing to his purpofe, he BEGAN to think for himfelf, and, *fortunately*, fell upon the following contrivance, which, he is happy to fay, has anfwered his purpofe extremely well.

From this it is evident, that all his actual knowledge of the difeafe, its caufes, confequences, and method of cure, was derived from this *fingle cafe*; for the fecond cafe he relates, came to his hands after his communication to you was written and ready to be fent off; of courfe, is only to be confidered (if I may adopt a parliamentary phrafe) as a rider placed upon the back of the original bill, in hopes it might add to the confequence of that, though itfelf had no pretenfions to any importance at all. And, confidering he has built all his fuperftructure upon this flender foundation, it is not very furprizing that he fhould fometimes be miftaken, often inaccurate, and not unfrequently obfcure.

I have been occupied upon this fubject almoft twenty years, during that time have feen feveral hundred cafes, including, perhaps, all the varieties of the difeafe, and am warranted in faying, I have never feen more than one cafe where the feet were turned inwards, and the bones of the legs *bent*; on the contrary, in all thofe cafes except the one I allude to, the bones of the legs were ftraight, in moft of them preternaturally fo.

As

As I have fully explained this fact in another place,* I content myself with asserting here that it is so, and hope your professional readers will examine more than one or two cases, before they regulate their opinions by the dictum of Mr. Watt.

Mr. W. says, "From whatever cause the distortion may proceed, the reducing *the bones*" (i. e. as I suppose, the parts concerned in those distortions) "to the natural position must be accomplished by nearly the same means, viz. by judicious pressure upon the convex side."

It is to be observed, that in those diseases where the feet turn inwards, and which, I presume, Mr. W. will call valgus, the deformity is occasioned by deranged combination of the bones of the foot, though in some the legs may be bent: that where the feet turn out, and I suppose these deformities are to be called varus, there is more frequently distortion of the bones of the leg, and the feet are frequently so pressed against the fibula while in utero, as to make the feet as flat as the hand of an adult; it is difficult to believe the same mode of treatment will cure both. If I may venture to add my mite to the intelligence of Mr. Watt, I should say, there is, in every case, one condition of the bones that will require peculiar treatment; there is another condition of the ligaments, and a third of the muscles; that each require treatment peculiarly adapted to them; and I can safely aver, that of more than an hundred cases which I have cured perfectly, no two were exactly alike, or could have been cured without adapting the mode of treatment peculiarly to each. If, after considering this fact, Mr. Watt will continue to say that all these diseases may be perfectly cured by being squeezed into the boxes he has described, I believe few intelligent practitioners will support his opinion. As all that Mr. Watt says, though *he is inclined to think there is no kind of distorted limb, however formidable it may appear, but may be cured by the same means*, depends upon what has been done in two (no doubt very slight) cases, and he has given *general accounts* of the diseases to which he proposes to apply the same mode of treatment, and which accounts are not very correct or just, it will be needless waste of time to follow him through the description of his plan; one particular, however, is too remarkable to pass unnoticed; he says, "*To keep the child quiet while we were putting on the instrument, and through the night, I found it necessary to use a gentle opiate once in twenty-four hours. This, however, did no hurt to the child's health; and as soon as the operation was finished, we gave it up entirely!*"

Mr.

* Practical Essay on the Club-foot.

Mr. Watt has said the cure was done with the greatest *ease*; now, if this was so, surely the regular use of opiates was unnecessary; if, on the contrary, as I have no doubt was the fact, Mr. Watt's squeezing system was so uneasy to the child, that he could not be kept quiet while the instrument was putting on, or obtain a single night's rest *without a gentle opiate once in twenty-four hours*, it was natural for Mr. W. to keep his patient under the influence of narcotics during the whole time he was performing the cure. The medical reader will accept this as a *strong proof of the ease of Mr. Watt's operation*; and it may induce him to entertain some doubts, even if there were no stronger reasons to doubt it, of the propriety of adopting a plan that includes *the necessity of keeping even an infant under the influence of* soporific drugs during the whole time of performing a cure.

Having shewn what he has done, Mr. Watt proceeds to shew what he thinks *may be done by his invention*; and as there are strong reasons for not misrepresenting what he has said on this subject, I trust I shall be excused for making the following extract from his description, &c.

He says, "*When I first thought of this instrument*, I had no idea of its being applied to any other case but the vari and valgi, and these only when the disorder happened to lie in the ancle joint. *But I have since thought*, that by a small alteration, it may be applied to all the different kinds of distorted limbs with considerable ease and advantage.

"Fig. 2, represents the half of the machine, in every respect similar to the one above described, save only, that one of its sides from the heel up is removed, *and in place of it a spring, the under end of which is fixed at* E, *to the side of the heel; the other, by means of the strap* F, *to the top of the machine. The convex side of the spring, stuffed and covered with soft leather, being applied to the convex side of the limb, by means of the strap* F, *any degree of pressure can be made, which the practitioner may judge necessary.* The side of the leg of the machine, opposite the spring, must have a head as represented at G, to rest against the outer or inner condyle of the femur, according as the case may require. *If the bones are bent forward, as is often the case, by fixing the spring upon the fore side, and causing it to press upon the convexity of the limb, they may be cured in the same manner as above directed.* By using a spring instead of straps round the limb, as directed by Mr. Benjamin Bell, a more uniform and effectual pressure can be made. *With regard to the strength of the spring, no general rule can be given; this must depend entirely upon the age of the patient, or the degree of force required to straighten the limb.*"

Thus

Thus far Mr. Watt. Will you excuse me, gentlemen, for informing you, that I have been employed many years upon this subject; having considered this class of diseases in a light they had never been placed in by any person whatever; having, by reiterated experiments, discovered and perfected a method of curing them much more effectual than any that had previously been known, I obtained a patent to secure the benefit of my discovery to myself for the usual term: In doing this, however, I did not incur Mr. Watt's censure by "concealing, as far as possible, my discovery from the public;" on the contrary, I resolved to make it as public as possible, from a conviction that by so doing I should extend its utility, and, of course, my own reputation.

In 1798, I published a Practical Essay on the Club-foot, &c. with a view to ascertain how far they were curable; and in that Essay I inserted a correct copy of the Specification of my Patent, with plates engraved from the drawings which accompanied it. As that Essay has acquired a character which it would not become me to describe, I shall content myself with making an extract from the specification contained in it, which extract relates to the subject in question. (*Practical Essay on Club-Foot*, p. 198.

"I shall illustrate the method of treating those distortions or deformities which arise from the improper form of bones, by explaining the treatment of curvature in bones of the leg, which is one of the most frequent diseases of that description; *whether the bones bend inwards, outwards, or forwards, is of no consequence, as the principle on which the remedy is to be applied is the same in all.* Figures 1 and 2, hereunto annexed, represent a child's leg, bending outwards; the lines marked with A, B, C, in both figures, represent the curved spring, intended to correct this deformity. It is evident, that if this spring is, by bandage or otherwise, at *a*, *d*, and *c*, *e*, in fig. 1, brought into contact with the leg, the inside of the knee, as at *d*, in figure 1, and *a*, in figure 2, and bottom of the leg, which correspond with the ends of the spring, will form resting points for the spring to act *from*; while its re-action, by producing pressure on the projecting part of the curve of the leg, reduces the bone towards its natural state. The same effect will be produced by either of these methods, the difference between the modes of producing the effect being, that in figure 1, the bandages at *a*, *d*, and *c*, *e*, form the resting points, and pressure is made upon the curve of the bone by the spring at *b*. But in figure 2, the ends of the spring, *a* and *c*, form the resting points, and the re-action of the spring produces pressure from the bandage, *b*, *d*, upon the curve of the bone."

 The

The resemblance between Mr. Watt's invention and mine is very evident, but the priority is indisputably with me.— Mr. W. may perhaps say, he has never seen my book; and, considering the distance of his residence from the metropolis, he may, perhaps, say so with truth: But it is to be remarked, that before the publication of my book, viz. in Sept. 1797, the proprietors of the Monthly Magazine inserted a detailed account of my Specification, with the explanatory plates, in their publication, which, from its very extensive circulation, it is most probable did fall into Mr. Watt's hands; though its contents made so little impression on his mind, that, at a subsequent period, he actually mistook the part I have quoted for the offspring of his own brain.

After saying thus much upon the resemblance of Mr. Watt's invention to mine, it is necessary to remark the difference between the inventors. Having by a series of experiments, followed for a length of time, which it would seem like affectation to dwell upon, discovered a new and effectual method of curing these diseases, I then made it public, with proofs that have incontestibly established its superior efficacy; but the effects of a long and successful practice, do not authorize me to assert, in the language of Mr. Watt, that "*there is no kind of distorted limb, however formidable it may appear*," but may be cured by the same means that have been successful in those cases that I have already cured. Mr. Watt, on the contrary, is accidentally called to a *single case*, which he knows not how to treat; he, naturally enough, began to turn over every book of Surgery within his reach, in hopes of gaining information, but without success; he then sets his own invention to work, and discovers *something* that proves successful; he immediately becomes omniscient on the subject, applies his invention, *in imagination only*, to numberless diseases of various kinds, and determines that all distorted limbs, however formidable they *may appear*, may be cured by the same means. Whence he derives his confidence, cannot be known; it certainly could not be from his knowledge and experience, as he had never seen but one case before he wrote the paper he has communicated to you, and one more before he sent it. This, to some of your readers, may appear absurd; it is not all the absurdity, however, contained in Mr. Watt's communication: For example, in his exordium he says, "Surgery, *so far as he knows*, has made little or no provision to cure the distorted limbs of infants;" and, *immediately afterwards*, he says, "though many have in other places remained, from whatever cause, in that situation for life; yet, *in this part of the country, I have not seen a single instance of one who has not been cured of the disease.*

It

It seems that Mr. Watt is not willing to raise the credit of his own country above its proper level; for if he was inclined to do it justice, and if the latter part of his assertion be true, he should have said, Surgery, in that part of the country, has done a great deal in the cure of these diseases.

As Mr. Watt justly censures the impropriety of resorting to itinerants, quacks, and *pretenders*, upon such subjects, and exposes the falshood of the opinion held by some, that regular surgeons can do nothing for the cure of these distortions, it is not a little surprising that when Mr. W. was called to the case of Wardrop, he had no knowledge of the proper method of treating it; it is *more* surprizing that notwithstanding the disease had been extirpated root and branch from all the surrounding country, (undoubtedly by the exertions of regular practitioners) no friendly practitioner was to be found in the neighbourhood, to inform him by what means the disease had been so perfectly and universally cured. This argument might be extended to greater lengths than I chuse to push it; for the train of ideas that Mr. W. indulges himself in, is too obvious to be mistaken by the most superficial reader.

Mr. Watt has laid much stress upon the saving clause, which he expresses by the words "*if taken in time*," with which he qualifies all his curative assertions; it is to be regretted that he has not afforded some criterion, by which the less experienced practitioner might have a chance to determine à priori, when it is not too late to undertake a cure of this kind with a probability of success. As he has not done this, however correct the ideas may be which he annexes to those words, they will afford a convenient subterfuge to palliate the want of success in every case; thus, if any person should attempt to practise that which Mr. Watt has only *thought must be universally successful*, IF TAKEN IN TIME, and finds he does not succeed, the failure must not be attributed to want of efficacy in the means employed, or want of knowledge and experience in the operator, but merely to the circumstance of the patient not having applied till it was too late to cure him; and thus, according to this system of logic, the method of cure may be good, if not a single patient should ever be cured by it.

As few intelligent men will believe, that Mr. Watt's system of squeezing the distorted feet of children into tin or leather boxes, &c. will be more successful than it has been, though attempted and recommended in various ways for many years past, that part of the subject may be left to its fate: But as the other part of his INVENTION, which he has only *thought* must be successful if applied to all the different kinds of distorted limbs, &c. so much resembles the description of

my invention, which was inserted with explanatory plates in the Monthly Magazine, that I shall believe it was copied therefrom, until there is strong proof to the contrary, may I be permitted to mention what my experience authorises me to say upon what Mr. Watt *conjectures* may be the consequence of applying *his invention* in such cases? It is this; if any person should apply the instrument described by Mr. Watt, to such cases as he there recommends it for, he will find it *perfectly useless*, because he will find it will not be possible to keep it so long in its proper situation as to produce any effect whatever.

I am to apologize for having obtruded longer upon your patience than I intended; but, I trust you are sensible that there is no circumstance more certain of producing mischief in any part of the healing art, than that of obtruding crude notions of any disease, and recommending modes of curing that disease as certain of being successful, though only founded upon some trifling experiment, and hope you may think the time not unusefully employed, that has been occupied in placing the importance and originality of Mr. Watt's communication in its just point of view.

Though a reader of your Journal from its commencement, I did not suppose this subject would have found a place in it; I now entertain a different opinion; and if you should think it deserving your attention, will trouble you with an account of some facts that have not been generally thought possible; among others I shall endeavour to ascertain at what period it is not too late to undertake the cure of distortions in the feet, &c. I shall prove that persons born with such defects, have been perfectly cured at the age of nine, ten, or eleven years; and I shall infer from thence, that many cases of this kind, which have long been abandoned as incurable, may be perfectly restored to their natural state: This, if you approve of it, will be the subject of my next communication. I am,

Gentlemen,

Your most obedient servant,

T. SHELDRAKE.

No. 50, Strand.

STATE

STATE OF DISEASES IN LONDON.

Diseases admitted under the Care of the Physicians of the Westminster Hospital, from the 23d of July, to the 20th of August, 1800.

Continued Fever	13
Intermittents	1
Pleurisy	1
Sore Throat	1
Enteritis	1
Amenorrhœa	2
Anasarca	3
Asthenia	9
Catarrh	1
Cholera	4
Cough	4
Diarrhœa	4
Enterodynia	2
Epilepsy	1
Gastrodynia	1
Hæmorrhois	2
Hydrops Ovarii	1
Hysteria	1
Jaundice	1
Lepra Græc	1
Menorrhagia	2
Opthalmia	2
Phthisis	4
Rheumatism	7
Vertigo	1
Urticaria	1

Account of Diseases in an Eastern District of London, from the 20th of July to the 20th of August, 1800.

	No. of Cases.
ACUTE DISEASES.	
Typhus	12
Ephemera	2
Dysentery	6
Cholera Morbus	15
CHRONIC DISEASES.	
Cough	7
Cough with Dyspnœa	3
Hoarseness	2
Hæmoptysis	4
Phthisis Pulmonalis	6
Dyspepsia	7
Gastrodynia	12
Bilious Diarrhœa	14
Colica Pictonum	2
Hæmatemesis	3
Hæmorrhois	4
Fluor Albus	3
Amenorrhœa	5
Menorrhagia	6
Cephalalgia	7
Hemiplegia	2
Chorea	1
Lumbago	1
Sciatic[illegible]	1
Chronic Rheumatism	6
Psora	2
Prurigo	5
Herpes	7
Tinea	2
Papulous Eruptions	12
PUERPERAL DISEASES.	
Mania	2
Low Puerperal Fever	3
Rhagas Papillæ	4
Menorrhagia Lochialis	6
INFANTILE DISEASES.	
Herpes Miliaris	7
Herpes Exedens	5
Diarrhœa	10
Aphthæ	5

The excessive heat of the weather, which has for some time prevailed, has been attended, as might be expected, with a considerable aggravation of the symptoms of many diseases. Instances of Typhus have been numerous, and very generally attended

attended with great determination to the head. Some patients labouring under this disease complained of confusion of ideas, whilst others were affected by considerable pain of the head; and in a few instances a high degree of delirium prevailed. In two puerperal cases the patients became maniacal; one of them recovered in a few weeks; but in the other case the disease terminated fatally.

Cholera Morbus has been very frequent, and in some instances particularly obstinate. The excessive discharge from the stomach and bowels which takes place in this disease, may at first view seem to require the use of powerful astringents to abate its violence; but the judicious practitioner is aware that considerable caution is necessary in the use of such means. Whilst opiates may be freely used to abate pain, and counteract the spasmodic affections of the lower extremities, if, under the use of them, a SUDDEN check is given to the discharge from the intestines, it sometimes becomes necessary to promote it by the use of some eccoprotic remedies. One instance occurred to the Reporter wherein a sudden, though SPONTANEOUS, cessation of the intestinal discharge was followed by such violent pain of the bowels as gave considerable alarm, and rendered the use of some active means necessary to restore the discharge, but which, as soon as it took place, was followed by a cessation of pain, and a gradual recovery to health.

MONTHLY REPORT of DISEASES,

Admitted under the Care of the PHYSICIANS of the FINSBURY DISPENSARY, St. John's Square, Clerkenwell.

The District, in which the Patients of the Finsbury Dispensary are visited, comprehends the Parishes of St. James, and of St. John, Clerkenwell; of St. Luke; of St. Sepulchre within and without; of St. Bartholomew, the Great and the Less; the Liberties of the Rolls, and of Glass-House Yard; the Town of Islington; the Parishes of St. Pancras; of St. Andrew, Holborn; and of St. George the Martyr, Queen's-square. This Tract of Ground may properly enough be termed, a North Western District of the Metropolis.

LIST of DISEASES, &c. from July 20, to August 20, 1800.

	No. of Cases.		No. of Cases.
Cholera and Diarrhœa	67	Amenorrhœa	15
Dysenteria	4	Leucorrhœa	6
Colica Pictonum	2	Rheumatism	9
Asthenia & Dyspepsia	17	Nephralgia	3
Prolapsus Ani	1	Catarrhus	13
Typhus	19	Pertussis	3
Phthisis	8	Cyn. Tonsil.	5
Menorrhagia	7	Cephalæa	4

Scrophula

Schrophula	8	Vermes	11
Anaſarca	6	Epiſtaxis	2
Pſora	1	Epilepſia	4
Hydrocephalus	3		

The above liſt will ſhow, that the typhoid contagion ſtill prevails in the Finſbury diſtrict, although with a degree of virulence that is by no means equal to that which it exhibited during the preceding month. Rarely has there been a ſeaſon in which this complaint has appeared ſo decidedly epidemic, and ſo troubleſome and alarming in its ſymptoms. The morbid affection of the brain was never, perhaps, a more uniform attendant of this fever, in conſequence, it is highly probable, of the unuſually intenſe and long-continued heat of the ſummer. One circumſtance denoting danger in Typhus, during hot weather, which has not hitherto been, at leaſt publicly, noticed, is the fixing of flies upon the face, neck, and arms of the patient, a circumſtance which may partly ariſe from his indiſpoſition or inability to drive them away; but ſtill more perhaps from a tendency to a cadaverous ſtate, as it is well known that theſe inſects have a particular attachment for bodies which the vital principle has either deſerted altogether, or on which its operation is in any conſiderable degree diminiſhed.

During this laſt fortnight the fever, vulgarly called putrid, has been gradually diſappearing, and Cholera, as is uſual in the autumnal period of the year, has become the popular diſorder. This is a complaint which requires more vigilance on the part of a medical attendant than perhaps any other by which the human conſtitution can be attacked, as it is not uncommon for it to carry off the patient, by its fatal violence, in the courſe of ſo ſhort a period as four and twenty hours.

W. W.
J. R.

No. 63, Hatton Garden.

Experiments, by Dr. Charles Darwin.

[Continued from p. 108, of our laſt.]

Exp. XVII. To a ſmall quantity of *coagulable lymph*, obtained by waſhing away the red globules from ſome craſſamentum of blood, I added about fifty drops of *acid of vitriol*; which changed the colour of the lymph to a reddiſh brown, but did not apparently diſſolve any of it.

Exp.

Exp. XVIII. To No. xvii. about an ounce of water was added without producing any obfervable change in the fluid, but the lymph after this feemed more denfe than before Experiment xvii.

Exp. XIX. One drachm of *cauftic lixivium* took five hours to diffolve the greater part of four grains of *coagulable lymph*; the remainder of the lymph had acquired a brown red colour, and was fofter than before it was added to the alkali.

Exp. XX. Water added to xix. produced a feparation of a whitifh fediment.

Exp. XXI. Equal parts of *ferum* and *cauftic lixivium* eafily united, and fuffered no change on the addition of eight or ten times their quantity of water; though after ftanding fome hours the mixture depofited a flight fediment.

Exp. XXII. Twenty-five drops of *vitriolic acid* added to one drachm of *ferum* converted it into a whitifh curdled matter.

Exp. XXIII. Water being added to No. xxii. the whitifh curdled matter was eafily difperfed through it; but foon formed a cloud near the top of the glafs. On the addition of more ferum to this mixture, it was converted into a fimilar matter.

Exp. XXIV. One drachm of *nitrous acid*, after fome time, diffolved three grains of *coagulable lymph*, with fumes and froth.

Exp. XXV. Water added to this, produced a dilute *green fluid*.

Exp. XXVI. *Acid of nitre* diffolved *mucus*, and with water occafioned a turbid dirty-coloured fluid.

Exp. XXVII. A faturated folution of *corrofive fublimate* coagulated *mucus* into a harder white material.

Exp. XXVIII. (This and the following Experiments were tried on Pus from a fuppurated mamma.) *Pus* diffufed readily through *pure water*, forming a white milky opake fluid, but after fome length of time fell down to the bottom of the glafs, where it had no great vifcidity; the fluid above it was for fome hours of a reddifh yellow colour, but became at laft colourlefs. By agitation the pus was again diffufible, but did not remain fo long fufpended as the firft time; and the fluid was immediately after its precipitation clear and colourlefs.

Exp. XXIX. *Acid of vitriol in fmall quantity* exhibited the fame appearances with this *pus* from a fuppurated mamma, as in No. 1, &c.

[To be continued.]

To

To Dr. BRADLEY.

SIR,

I Have ſent you four hoſpital caſes of Diabetes Mellitus, three of which have been ſucceſsfully treated, and the other in ſome degree relieved, by the adminiſtration of the Nitrous Acid largely diluted. They are copied from a book wherein I make minutes of all the caſes which come under my care. If you think they deſerve a place in your periodical Work, you are at liberty to inſert them.

The urine of Giſſing and Maſon left a good deal of ſaccharine matter after evaporation, which was of a very dark colour, and of the conſiſtence of very thick treacle. I do not recollect, neither can I aſcertain from the minutes, what proportion the quantity of ſaccharine reſiduum bore to the quantity of urine ſubmitted to the experiment. I ſhould have made ſeveral more experiments upon the urine of the different patients if I had not been too much engaged at the time.

Mr. Chavaſſe, of Walſall, a gentleman of eminence in his profeſſion, has juſt ſent me a caſe of Diabetes Mellitus ſucceſsfully treated by himſelf, which I have alſo incloſed, and which I ſhall be obliged to you to make public.

I am, SIR,

Your obedient humble ſervant,

July 31, 1800. WILLIAM GILBY,

Phyſician to the General Hoſpital, near Birmingham.

CASE I.

Anthony Giſſing, aged 42, (admitted January 17,) complains of great weakneſs and inceſſant thirſt. Tongue dry and parched, particularly in a morning; ſkin dry, and harſh to the touch; feels great fulneſs of the abdomen, which is hard and viſibly enlarged; makes 14 pints of urine (which is ſweet and of a pale colour) in the 24 hours, although his drink during this period does not exceed 10 pints; pulſe 90; bowels coſtive; appetite not good, which he attributes to his having lived almoſt entirely upon animal food for ſome months paſt; ſays that he has loſt fleſh very much within the laſt half year.

Preſcription, with reſpect to diet, to live more upon animal than vegetable food, and to have, inſtead of malt liquor, two ounces of brandy mixed with water twice a day.

Capiat ſeri aluminoſi ℥iv, ſextâ quâque horâ et pulveris rhei gr. xv. pro re nata ex quovis vehiculo idoneo.

Jan. 23. Says that he feels himſelf weaker ſince he came into

into the hoſpital, and that the urine, with reſpect to quantity, taſte, and colour, is the ſame; thirſt increaſed, but has refrained from drinking, as he did not wiſh to exceed 10 pints in the 24 hours; bowels ſtill in a coſtive ſtate.

℞. Decoct. hordei (additis ſub finem coctionis rad. glycyrrhiz. incis. ℥iij } ℔ ij.
Acidi nitroſi) - - - - - - - - ℨj.
Fiat miſt. cujus coch. v. vel vi. ter quaterve intra viginti quatuor horas capiat.

Jan. 26. Reports that he has taken 10 pints of liquids, and made 12 pints of urine, ſince the morning of yeſterday.

Capiat miſt. acid. coch. vj. ſextâ quâque horâ.

Jan. 28. Drink, 9½ pints in 24 hours; urine, 11 pints, which is ſtill pale coloured and ſweet; feels leſs pain in the abdomen, which, upon examination, is not ſo hard, nor ſo much ſwelled as it was at the time of his admiſſion.

Jan. 30. Drink, 9 pints; urine, 10 pints; complains of pain in his ſtomach and bowels. The urine is of a better colour, and leſs ſweet.

Omittantur aliquot doſes miſturæ acidæ. Capiat tinct. opii camphorat. ℨj. vel ℨjſs. urg. ventric. vel inteſtinorum dolore ex infus. flor. cham. poculo.

Feb. 2. Drink, 8 pints; urine, 9 pints; belly much ſofter. The pain having gone off, he yeſterday took eight ſpoons full of the acid mixture for a doſe.

Sumat miſt. acid. doſin ſextis horis et bibat ſubinde infus. Cinchonæ flavæ poculum.

Feb. 7. Drink, 8 pints; urine, 7 pints, which is not ſweet; thirſt abated, but is yet troubleſome; pulſe 80.

Feb. 9. Drink, 7 pints; urine, 8 pints, which has had no ſaccharine taſte for the laſt five days; feels ſtronger; ſays he thinks he ſhall recover, as the urine has loſt its ſweetneſs.

Adde miſturæ acidi nitroſi ℨſs. capiat uncias quatuor quintâ quâque horâ.

Feb. 14. The report the ſame as on the 9th, except that the urine is rather leſs in quantity.

Feb. 18. Drink, 7 pints; urine, 7 pints.

Feb. 23. Drink, 6 pints; urine, 6½ ditto.

Feb. 27. Report the ſame as on the 23d; bowels ſtill coſtive, but free from pain.

Capiat pulv. rhei. gr. xv. ex infus. ſennæ ℥ij. pro re nata.

March 6. Drink, 5 pints; urine, 6 pints.

Loco ſp. vin. gallic. habeat cereviſiæ fortioris ℔ j. quotidie.

Mar. 15. Report as on the 6th.

Augeatur quantitas cereviſiæ ad ℔ ij. doſin miſt acid. ſpatio horarum octo ſemel tantummodo capiat.

Mar.

Mar. 21. Drink and urine the ſame as on the 6th; feels himſelf conſiderably ſtronger, ſince he has had a larger allowance of ale. The urine made in the night is ſometimes a little ſweet.

April 1. Drink, 6 pints; urine, near 7 pints; complains of pain in the ſtomach and bowels.

Capiat opii gr. ſs. cum doſibus miſturæ ſingulis.

April 8. Report reſpecting the urine the ſame as on the 1ſt inſtant. Pain continues.

Capiat opii gr. j. nocte maneque cum miſt. acidæ unciis quatuor.

April 15. Drink, 6 pints; urine, 6 pints.

Diſcharged at his own requeſt.

This patient might have gone on better if he had not been guilty of ſome irregularities. He obtained leave to go into the town one afternoon, got intoxicated, and experienced the uſual ſymptoms of debility afterwards. His thirſt increaſed; and he had, for a day or two, an increaſed diſcharge of urine. At the time he left the Hoſpital the urine had its proper colour, taſte, and ſmell. I wiſhed him to remain ſome time longer; but as he had a family, and had regained his ſtrength, he wiſhed to return to his work. He was deſired to ſend an account of himſelf at the end of a month, and was promiſed a further ſupply of medicine if the urine ſhould at that time exceed 4 pints in the 24 hours. No account has ever been received either by the apothecary or myſelf. He lives many miles from Birmingham, and I am unacquainted with his addreſs.

CASE II.

Mary Jeſſop, aged 43, admitted Feb. 20th, complains of great thirſt, and loſs of appetite. Tongue dry, and very much furred, eſpecially in a morning; has great heat at her ſtomach, and is frequently troubled with bilious eructations. Says that her belly has become ſore and enlarged within the laſt ſix months. Makes betwixt nine and ten pints of urine in the 24 hours, which is ſweet, and of a very pale colour. The quantity of liquids taken within this period, ſhe reports to be about four pints and a half. Has a conſtant pain in her legs and thighs. Pulſe near 100. Stools regular. Has had no menſtrual diſcharge for ſome months paſt.

Capiat miſt. acid. ut pro Giſſing præſcript. coch. iij. vel iv. octavâ quaque horâ.

Feb. 23. Drink, 4 pints; urine, 8 pints, ſince the morning of yeſterday, when ſhe began to take ſix ſpoons full of the acid mixture for a doſe.

Adde misturæ acidi nitrosi ʒss. capt. coch. vi. sextâ quâque horâ.

Apr. 25. Drink, 4 pints; urine, 7 pints; in the 24 hours.

Apr. 27. Drink, 4 pints; urine, 6 pints.

March 3. Drink, 4 pints; urine, 5 pints. Pulse 84; pains in the legs and thighs nearly gone; belly less swelled, and free from soreness; thirst abated; urine, made in the day, not sweet, and of a much better colour.

March 8. Report respecting the drink and urine the same as on the 3d. Bowels costive.

℞. Pulv. rhei. ʒiss infus. sennæ ℔ss. M. cap. coch. iij vel iv pro re nata.

Mar. 15. Drink, 3½ pints; urine, 5 pints, which is seldom sweet and of a proper colour. Has had several purging stools in the night. Omittatur mist. acida.

℞. Mist. cretacea ℥xiv. sp. cinamom. ℥ij. M. cap. coch. vi ter quaterve die donec alvus comprimatur. Capiat horâ decubitus opii gr. ss.

Mar. 21. Drink, 4 pints; urine, 4½ pints. Bowels easy, stools regular.

Mar. 27. Drink, 3½ pints; urine, 4 pints, which has no sweet taste, and is of a proper colour. Says she feels very comfortable, and wishes to be discharged.

Jessop was so much reduced by the disease, that it was quite a fatigue to her to go up stairs, or even move about the house; to use her own words, "She had no power to do any thing." She had been under my care several months before she came into the hospital, had lived as much as possible upon animal food, abstained from all fermented liquors, and had taken most of the remedies usually given in a Diabetes, but without deriving any benefit. At the time of her admission, I directed her to take the common diet of the house, but to abstain from malt liquor; as a substitute for which, she was allowed to have, now and then, a little very weak brandy and water. This soon became disagreeable to her, and orders were given that she should have half a pint of ale two or three times a day. After she had been in the house about ten days, her appetite was much improved, digestion tolerably good, and she was able to walk about the ward with very little fatigue.

CASE III.

John Nichols, aged 45, admitted an out-patient on May 2; complains of great thirst, soreness, and fulness of the abdomen; makes ten pints of urine in the 24 hours, which is sweet, and of a very pale colour, but is uncertain as to the quantity of liquids

quids which he takes during this period; thinks it does not exceed five pints. Pulſe 86. Says he takes a great deal of food without being ſatisfied.

℞. Acidi nitroſi ʒjſs. decoct. hordei ℔jſs. ſyr. ſimp. ℥ij ft. Miſtura cujus coch. iv. cum aquæ eadem quantitate ter die capiat.

May 13. Liquids drank, 4 pints; urine made, 6 pints. Says he has no ſoreneſs in his belly, very little thirſt compared with what he had ten or twelve days ago. Urine of a better colour, and leſs ſweet.

May 20. Report nearly the ſame as on the 13th; feels much ſtronger; thinks he is able to work.

Capiat miſt. acid. ut antea et bibat decoct. cinchonæ flavæ uncias tres bis die.

May 27. Liquids taken, 3 ½ pints; urine, not 6 pints. Abdomen ſoft, and free from ſoreneſs; thirſt abated; pulſe under 80.

June 3. Liquids taken, near 4 pints; urine 5 pints, which is of a proper colour, and never ſweet. The fulneſs of the abdomen is ſo far gone, that the waiſtcoat which he wore at the time of his admiſſion, and which was then very tight, is now much too wide.

June 10. Liquids drank, near 4 pints; urine, 5 pints. Says he feels nearly well, but not ſtrong enough to go through a day's work.

Omittantur miſt. acid. et decoct. cinchonæ.

℞. Infus. gentianæ comp. ℔j. Acidi vitriolici diluti ʒij. M. cap. coch. iv ter die.

July 13. Called upon me this morning, and ſays that his health was never better. He cannot ſpeak as to the quantity of urine he makes, but knows that it does not exceed what it ought to be.

Nichols never made any alteration in his diet, and never drank any other thing than ſmall beer and cider.

CASE IV.

John Maſon, aged 23, (admitted January 24) complains of great weakneſs; ſays he has a moſt voracious appetite, and a continual thirſt; makes eleven pints of very ſweet, pale-coloured urine in the twenty-four hours, but cannot ſay what quantity of liquids he takes during this period. Appears much emaciated; pulſe near 100; bowels coſtive.

Capiat miſt. acid. ut pro Giffing præſcriptæ coch. iv ſextis horis.

Jan. 28. Liquids drank, 7 pints; urine made, 11 pints, ſince

since the morning of yesterday, which is still pale coloured and sweet.

Capiat infus. sennæ uncias tres quatuorve quoties alvus sit astricta.

Feb. 4. Report with respect to the urine, &c. the same as on the 28th ult.

Feb. 9. Liquids taken, 7 pints; urine made, 10 pints. Has pains in his stomach and bowels.

Capiat opii pur. gr. ss. urg. dolore.

Feb. 18. Report the same as on the 9th, except that he feels little or no pain. Urine sweet, and of a pale colour.

Capiat mist. acid. uncias quatuor quintâ quâque horâ.

Feb. 20. Report as on the 18th, except that the urine made in the day has a salt taste, and is of a higher or deeper colour.

Adde mist. acid. nitrorsi ʒss capiat dosin eadem quæ 18vo. erat præscripta.

Feb. 23. Liquids drank, 7 pints; urine, $9\frac{1}{2}$ pints, which is of a much darker colour, and seldom sweet. Legs œdematous in the evening.

Feb. 27. Liquids drank, 7 pints; urine, 9 pints.

March 6. Report as on the 27th ult.

Mar. 11. Liquids drank, 7 pints; urine, $8\frac{3}{4}$ pints. Complains of great weakness. Appetite bad; pulse 86.

Habeat cerevisiæ fortioris ℔ss ter die.

Mar. 18. Liquids drank, $6\frac{1}{2}$ pints; urine, 9 pints, which is sometimes sweet and at others salt. Says that his nights are bad, owing to griping pains, occasioned, he thinks, by the acid mixture.

Capiat h. s. opii gr. j vel gr. jss.

Mar. 25. Liquids taken, $6\frac{1}{2}$ pints; urine, $8\frac{1}{2}$ pints.

April 1. Liquids taken, 7 pints; urine, $9\frac{1}{2}$ pints.

Ap. 8. Report as on the 1st. Wishes to give over taking medicines, and to try whether a month's residence in the country will be of any service, as he begins to loathe all kinds of food.

Omittatur mist. acid. Bibat aquæ calcis uncias quatuor quartis horis.

Ap. 15. Liquids taken, 7 pints; urine, 9 pints.

Discharged.

May 13. Has been drinking the lime-water ever since the 15th of last month; and says that his urine is of a pale colour, and sweet; and that he generally makes about ten pints in the 24 hours. Appetite is more keen since he left the hospital.

The acid medicine has not done much for this patient. He lived for some weeks almost entirely upon animal food, and then

then ſaid it went ſo much againſt his ſtomach that he could not get down any more, unleſs he were to have an allowance of bread and malt liquor.

Caſe of Diabetes Mellitus ſucceſsfully treated by the Nitric Acid; communicated to Dr. Gilby *by* Nicholas Chavasse, Surgeon in Walſall, and Member of the Company of Surgeons in London.

CASE.

Thomas Pitt, aged 64, applied to me about ten weeks ſince, with an inveterate Diabetes. He had been a hard drinker for more than twenty years, and had laboured under a profuſe diſcharge of water for thirteen months. When he firſt conſulted me, he was making nine quarts of ſweet urine in twenty-four hours: His pulſe was weak and frequent; his thirſt exceſſive; appetite very bad, and appearance cachectic. He was directed to live upon animal food, to abſtain from vegetables, ſugar, and all fermenting liquors; but at any time, when unuſually faint, to drink a few glaſſes of Sherry. He took volatile and fixed alkali alternately in the day, and opium at bed-time. As the latter medicine produced coſtiveneſs, he had recourſe to the flowers of ſulphur to remove that inconvenience. He perſiſted in following theſe rules for twelve days; but as œdematous ſwelling of the lower extremities came on, accompanied with increaſed debility, hectic fever, and urinous ſecretion, I thought it prudent to abandon this plan. He was now directed to take every two hours, two ounces of a mixture, compoſed of a drachm of nitric acid, a quart of water, and one ounce of the ſyrup of ginger; to relinquiſh the uſe of his opium; to eat any kind of food his ſtomach would prefer; to drink porter at his meals, and four glaſſes of Port wine after dinner. At the expiration of a week, he called on me again; I found his general health much improved, and the quantity of urine evacuated was diminiſhed to ſeven quarts. Fearing the acid might irritate the bowels, the uſe of it was reduced to every four hours; and he continued taking that doſe for twelve days longer, when I found him convaleſcent, and making only five quarts of water in the day and night. Suſpecting that this favourable change might be the reſult of a change of diet, I directed him to eat and drink as he had done the laſt nineteen days, to diſcontinue all medicines, and to call on me again in a week. I conjectured erroneouſly, for five days had ſcarcely elapſed, when the anaſarcous ſymptoms, and increaſed ſecretion of urine both returne.d He then regularly

gularly perſiſted in taking the nitric acid every four hours for a month; when the efficacy of the medicine was as conſpicuous as before. He now enjoys better health than he has done for years, and his flow of urine is as moderate as it ever had been. Now we are upon the ſubject of nitric acid, permit me to ſay, I cannot, with truth, unite my evidence to thoſe who have given ſuch flattering accounts of its efficacy in the cure of inveterate ſyphilis; I have tried it, and fairly tried it, in a great number of caſes of lues, in different ſtages of the diſeaſe; and the reſult of all my experience convinces me, that it has no ſpecific powers in curing that diſeaſe. When the conſtitution has been broken down by violent pain, and the exceſſive uſe of mercury, then I know it will relieve local ſymptoms, merely by improving the general health: But here, the mercury muſt be laid aſide, and the ſame good effects may be gained by the vitriolic acid, the bark, or both united; or by ſteel medicines; all of which we know have a tendency to invigorate the digeſtive organs. I do not find, by comparative trials, that the nitric acid poſſeſſes any ſuperiority over the medicines I have enumerated.

I ſhall be happy to find, that the experience of other practitioners will enable them to ſpeak as highly of the nitric acid in the cure of Diabetes as I, with truth, have done in this ſolitary caſe.

To the Editors of the Medical and Phyſical Journal.

Gentlemen,

Your valuable and truly uſeful Journal needs no encomiums of mine to point out its various merits to an enlightened and diſcerning public; its extenſive circulation through the Britiſh dominions, and the great reſpectability of many of your Correſpondents, among whom I am happy to notice the name of Dr. Denman, are the ſureſt pledges of its ſtability and increaſing celebrity.

The readineſs with which you communicate to the public all facts and obſervations that have a tendency to enlarge the boundaries of Medical Science, or alleviate the miſeries of human nature, induces me to requeſt the inſertion of the following caſes of ſucceſsful treatment of Dropſy, in your next monthly Miſcellany.

In the plan of treatment and cure adopted in theſe caſes, I have

have not to boaſt the diſcovery of any new remedy, either chemical or botanical; no new ſpecific to propoſe, or arcanum to lay open; in this reſpect, the lovers of novelty and wonder will certainly be diſappointed: But although the medicines employed have been long known and eſteemed almoſt by every Tyro, as well as by men of the firſt ability in the profeſſion, yet it muſt be admitted, that when theſe are united, and variouſly combined with no properties chemically repugnant, they may poſſeſs energies and active powers, which ſeparately and diſtinctly adminiſtered, they would wholly fail in.

In the cure of every ſpecies of Dropſy, practitioners in general have two indications in view, that of evacuating the ſuperabundant fluid in the firſt inſtance; and, ſecondly, to guard againſt its farther accumulation. To accompliſh theſe deſirable ends, recourſe is almoſt invariably had to the operation of draſtic purges, frequently exhibited and repeated, ſuch as ſeneka, jalap and calomel, the neutral ſalts, and the occaſional uſe of diuretics. When by a due perſeverance in theſe means, the water is totally or in great meaſure expelled, different tonics, as bark, ſteel, and bitters are employed to reſtore the loſt tone of the conſtitution, and confirm the cure. But although this treatment may have frequently ſucceeded in removing the diſorder in queſtion, yet there is great reaſon to believe that it has been only in young and naturally robuſt habits, where the complaint was of recent ſtanding, and not depending on any previouſly exiſting diſtemper. For, if we are to conſider Dropſy as a diſeaſe commonly of high debility, and conſequently of diminiſhed excitement, whether originally produced by profuſe evacuations of blood, ſuddenly inducing a conſiderable diminution of the vital powers, or by long protracted acute diſeaſes, or other cauſes occaſioning general atony and relaxation of the fibres, we ſhall naturally and juſtly be led to conclude, that theſe violent evacuant remedies have rather a tendency to increaſe the malady by ſtill farther exhauſting the excitability or ſenſorial power, than to remove it.

That this practice is often attended with great danger, and ſometimes with fatal conſequences, we have the teſtimony of Dr. Milman, who ſays, that after the Dropſy is removed, the patients will ſometimes die without any evident cauſe; and again, this reſpectable author obſerves, that we muſt take care not to make too free with the uſe of purgatives if the patient be weak. I might aſk the Doctor with much propriety, if he had ever met an inſtance of dropſy, aſcites, or anaſarca, as a general diſeaſe, that was not accompanied with and preceded by great debility? Notwithſtanding theſe cautions, and their manifeſt propriety, the Doctor however depends, as Sydenham

and others did before him, chiefly on the good effects of purgatives; for he speaks, among other things, of the advantages arising from a decoction of seneka procuring nine or ten stools a day. Sydenham advises cathartics to be administered every day, unless through the too great weakness of the body, or the violent operation of the purgative, it shall be necessary to interpose a day or two now and then; because, he says, if considerable intervals be allowed to occur between the exhibition of the purgatives, the water will be afforded an opportunity of again collecting.

A careful and diligent investigation into all the phenomena attending Dropsy, will convince every impartial and unbiassed observer, that it is always a disease of diminished, and never of increased excitement; unless when excitement is pushed beyond its due bounds into indirect debility, as is frequently the case with men who indulge too freely in the luxuries and conviviality of the table, whose dissolution is not unusually preceded by this very disease: But still debility, whether of the direct or indirect species, and however induced, must precede and accompany it throughout all its stages.

If this view of the question be just, and founded on accurate observations deduced from the phenomena known invariably to attend the disease, how many lives, then, must annually have fallen a sacrifice to the common but mistaken practice? a practice grounded on false and erroneous theory; a practice which, instead of opposing the destructive tendency of the disease, is in direct unison with it; an accredited error sanctioned by ages, and maintained on the authority of great names, but which will be discarded the moment we have courage to reason from effects to causes, and to consider real facts divested of the influence of splendid but illusive systems. I am unacquainted with any cause which has operated more powerfully against the extension and improvement of medical science, than implicit confidence in certain illustrious names and systems of physic: The mind resting with a kind of fatal security on the supposed truths of those oracles, suspends its own inquisitive propensities and energies, thereby effectually shutting up all avenues to enquiry and investigation.

Convinced by experience of the inefficacy and pernicious consequences of employing evacuant and debilitating remedies for the cure of a disease depending on atony and relaxation, more especially of the vascular system, I pursued a course diametrically opposite.—I endeavoured to support and invigorate my patients by the most powerful and permanent stimuli, both with respect to medicines and diet; and with these were joined active diuretics, in order to stimulate the kidneys and determine

mine the ſerous fluid to thoſe paſſages. In ſhort, to ſtrengthen the habit, to reſtore the energy and loſt tone of the vaſcular ſyſtem, to promote abſorption, and finally to expel the ſerous collections from the body, were the ends propoſed: how far I ſucceeded will be beſt ſeen by a narrative of the following caſes.

CASE I.

It is to be recollected that I began the treatment of this caſe on the old plan. James Griebel, a French priſoner, a young man 28 years of age, was admitted into the Hoſpital at Norman Croſs on the 3d of November 1799; he dated the commencement of his complaint about three weeks before, occaſioned probably by poverty and want. On examination, I found the abdomen very much enlarged with evident fluctuation, the legs much ſwelled and œdematous, eaſily receiving and retaining a long time every impreſſion; a quick, ſmall pulſe, a dry tongue, parched ſkin, great thirſt; paſſed little or ſcarcely any urine; a ſhort troubleſome cough; difficult reſpiration, ſo much ſo that he was obliged to keep in a half erect poſture whenever he ſlept; a ſallow, emaciated countenance, with large blotches and ſcabby eruptions all over the body. He was immediately ordered a briſk cathartic of jalap and calomel, which procured ſeveral copious ſtools and relieved his breathing. On the 5th the ſymptoms became equally oppreſſive and diſtreſſing. Rep. cathar. with ſimilar effects, though leſs permanent. 6th, All the ſymptoms recurred with additional violence, the patient being conſiderably reduced. Ordered this day a diuretic mixture, Aq. menth. kali, acet. et tin. ſcillæ. Paſſed no water except when at the water-cloſet, and then even very ſcantily. 8th, Several watery evacuations by the anus yeſterday; notwithſtanding which, the legs, thighs, and abdomen were more enlarged than ever, with extreme debility and emaciation. It now became evident that the patient would ſpeedily ſink, unleſs relieved by other means. Under this idea he began the following mixture this day: ℞. Aq. menth. ℥vj. ferr. vitriol. kali. myrrh an. ℈j. tin. ſcillæ. ſpt. æth. nit. a. ʒiſs. tinct. opii. gt. xl. ſumat cochl. iij. 4is. horis. At the ſame time he was ordered a pint of red wine, and nouriſhing diet in ſmall quantities frequently repeated. 9th and 10th, no alteration. On the 11th he made conſiderably more water, but he was ſtill greatly debilitated. 12th and 13th, the evacuations by the kidneys much increaſed, ſpirits better, ſtrength amended. 15th, much better. The power of the mixture augmented: Ferr. vitr. kali. myrrh a. ℈ij. tinct. op. gt. l. tin. ſcillæ. ſpt. æth. n. a. ʒiij. aq. menth. ℥vj. ut antea. S. 17, Quantity of urine now daily 5 pints. 18th, Increaſed to 6 pints, of a duſky brown colour with much ſediment;

ment; treatment the ſame; all the bad ſymptoms greatly abated; the legs, thighs, and abdomen conſiderably reduced; reſpiration much relieved, thirſt gone, appetite good; in ſhort, he was wonderfully amended. 23d, Almoſt free from complaint, paſſing daily 6 pints and upwards of urine. His medicines and treatment continued the ſame till the 30th, at which time he was perfectly reſtored to health; and on the 4th of December he was diſcharged to priſon, having paſſed during the laſt 15 days near 90 pints of water, beſides what was loſt during operations by the anus. It is worthy of remark, that the tone and vigour of the fibres of the inteſtinal canal were by this time ſo completely reſtored, that he was obliged to take aperients twice, on the 24th and 29th. The ſcabby eruptions had at firſt much the appearance of being a ſpecies of itch; but before the 30th the ſkin was wholly cleared from them.

CASE II.

John Colquhoun, aged 42 years, a ſeaman belonging to his Majeſty's ſhip Le Pompée, was admitted into the Royal Hoſpital at Plymouth on the 4th of June laſt, labouring under univerſal anaſarca; his legs and thighs were ſo aſtoniſhingly diſtended, that it was with the utmoſt difficulty he could ſtand; great proſtration of ſtrength, dejection of ſpirits, a troubleſome cough with expectoration, thirſt, difficult reſpiration, pain about the ſcrob. cordis, loſs of appetite, profuſe diarrhœa; paſſes no urine except at ſtool, and then only a few drops at a time. The hiſtory of the caſe previous to admiſſion: He had been affected with cough at leaſt twelve months; the ſwellings of the extremities began about five weeks before I ſaw him, and the diarrhœa was chiefly occaſioned by the purgatives employed to remove the dropſy; he was, upon the whole, much emaciated and reduced. The ſtate of his bowels became the firſt object of my care; and having by the uſe of the cretaceous mixture, tinct. catechu and opium, almoſt removed this complaint, he began the following mixture on the 7th: ℞. Ferr. vitriol. grs. xv. kali et myrrh a. ℈j. aq. menth. ℥vj. tinct. ſcillæ. ſpt. æther nitr. a. ʒiſs. tinct. opii gts. lx. M. capt. cochl. iij. 4is horis. This treatment was continued till the 11th, with little amendment. ℞. Aq. menth. ℥vj. ferr. vitr. ʒſs. tinct. ſcil. ſpt. æth. n. a. ʒijſs. tin. opii. gt. lxx. M. ut antea ſumend. wine, nouriſhing diet and drink without reſtriction. 13th, The diſcharge of urine conſiderable. 20th, The quantity amounted to about 5 pints, and continued to increaſe daily. 26th, The ſtrength of the mixture ſtill farther augmented to, Ferr. vitriol ℈ij. kali et myrrh a. ℈ijſs. tinct. ſcill. ſpt. æth. n. a. ʒiij. tinct. opii gt. lxx. aq. menth. ℥vj. M. From this time

time the discharge by the kidneys increased daily, till it amounted to 6 pints and upwards of high coloured water, emitting a strong fœtor, and depositing a heavy sediment. On July 10, his extremities were reduced to their natural size, the cough and expectoration almost wholly removed, the pain about the scrob. cord. much abated, and his strength and appetite greatly amended. On the 11th, he had several copious motions; diarrhœa being extremely prevalent among the patients for some time past, which compelled me to suspend the Chalybeate mixture, and to have recourse to the cretaceous as at first. In four days the diarrhœa was removed; and, on the 15th, he commenced the Chalybeate mixture, although every vestige of Dropsy was at this time wholly removed; but the diarrhœa had again reduced him. It is remarkable, that during the 13th, 14th, and 15th, he scarcely made a quart of water each day.

This man was accidentally seen on board the Pompée, and ordered to the Hospital by Dr. Harness, Commissioner of sick and wounded Seamen; a gentleman, the suavity and politeness of whose manners are only equalled by his humanity and professional abilities. From the marked appearance of hectic, joined to the anasarca, diarrhœa, and other bad symptoms attending this patient, the Doctor considered it a lost case.

CASE III.

Henry Roach, 26 years of age, a seaman belonging to his Majesty's ship Windsor Castle, was received into the Royal Hospital on the 1st of June, 1800. I found the abdomen enormously enlarged, fluctuation, dry tongue, unquenchable thirst, the lower extremities much tumified and œdematous, an unceasing cough, and he expectorated largely a purulent kind of mucus, if I may be permitted the expression, slightly tinged or streaked with blood; a fixed and distressing pain about the scorbicul. cordis, attended with profuse perspirations daily towards morning. The abdomen was so distended, and his strength so exhausted, that he could neither stand on his legs from the size and weight of his body, or lie down for fear of suffocation. He dated the commencement of his disorder from having put on a damp shirt the 24th of February last; from that time he had constant cough, which gradually increased till his belly began to swell, about five weeks before his admission. Whatever cathartics could do had been already liberally tried on board ship. His countenance had all the appearance of a completely formed hectic; the alæ of the nose were pinched in, the cheeks sunk and hollow, with a circular rouge-like tinge in the centre of each; in short, his whole appearance indicated

approaching

approaching diſſolution, or, at beſt, incurable phthiſis; nor had I the ſmalleſt hope of his recovery. For the firſt three days I preſcribed only a pectoral mixture with tinct. opii camp. to eaſe the violence of the cough, expecting that he could not ſurvive many days. On the 4th of June, however, he began with the Chalybeate diuretic mixture, which was continued till the 8th without much variation; but he made on this day near a quart of water at different times. Increaſed the ferr. vitri. to gr. xxv. kali. myrrh a. ʒſs. tinc. ſcillæ. ſpt. æth. nit. a. ʒiiſs. tinc. op. gt. xl. aq. menth. ℥vj. m. ſum. cochlear iij. 3is horis. Allowed a pint of red wine; no reſtrictions reſpecting the quantity of common drink, which was made by boiling ʒij. of crem. tart. for a few minutes in a quart of water with a little orange-peel, and ſweetened with honey. From this time to the 26th there was no alteration in the treatment, but the diſcharge by the kidneys increaſed to the aſtoniſhing quantity of between 8 and 9 pints of water daily, very high coloured, and depoſiting a copious ſediment; he began now to recover rapidly. Increaſed the ferr. vitr. to gr. xxx. kali myrrh a. ℈ij. tinc. ſcil. ſpt. æth. nit. aa. ʒiijſs. aq. menth. ℥vi. M. This quantity was daily perſevered in till the 13th of July, on which day he quitted the Hoſpital perfectly reſtored to health, all the ſymptoms having gradually diſappeared; even the cough and expectoration appeared wholly removed. There was a variety of ſmall occurrences reſpecting this and the preceding patient, which I have omitted, to avoid prolixity; but each had bliſters applied to the ſcrob. cord.

From the fortunate termination of theſe bad caſes, are we not juſtly entitled to conclude, that the above plan of cure will commonly prove ſucceſsful for the removal of Dropſy as a general diſeaſe, and even when combined with incipient phthiſis, or a morbid ſtate of the lungs? But where it depends on long eſtabliſhed ſcirrhus of the liver, or other viſcera, on polypi of the heart and great blood veſſels, on compreſſion from fixed and indolent tumours, or, in ſhort, when it is ſymptomatic only of ſome internal incurable malady; then, indeed, the probability of ſucceſs will be leſs apparent. I am,

Gentlemen,

Your obedient humble ſervant,

July 20, 1800. JAMES MAGENNIS, M. D.

Phyſician to the Royal Hoſpital, Plymouth.

To Dr. DYCE, Aberdeen.

Dear Sir,

I Have obſerved in the courſe of my reading and practice, that Hydrocephalus Internus has always been attended with fatal conſequences. It is, therefore, in my opinion, the duty of every practitioner to contribute as much as is in his power to the elucidation of a diſeaſe ſo fatal to the human race. I ſubmit to your conſideration the following caſe, which lately came under my care, and which differs conſiderably from all others that I have yet met with; if you think that it deſerves a place in the Medical and Phyſical Journal, I beg leave to requeſt the favour of you to tranſmit it to the Conductors of that valuable publication.

About the end of May laſt, I was called to ſee a child about ſeven weeks old; I found him very emaciated and weak; his head much enlarged, and the ſutures conſiderably ſeparated. From theſe ſymptoms, I was led to conclude, that there was water in his head. His appearance and ſituation ſeemed to indicate that he could live only a few days; I therefore did not attempt any cure, but was induced to enquire particularly what ſtate he was in prior to my ſeeing him. I was informed, that he was a ſtout healthy child when he was a month old, and that his head had no uncommon appearance. He took ſevere fits of crying every afternoon for the four firſt weeks of his age, which was attributed to gripes: he generally made leſs water than is uſual for children at his time of life, and always foamed at the mouth when aſleep. In general, he reſted very ill; but ſlept better in the forenoon than at any other time. He had a good appetite; his food was flour bread and milk, but he vomited whenever he took bread and water: he was always regular and eaſy in his bowels. When a month old, he was ſeized with a fever, which laſted two weeks, and during its continuance, he cried almoſt day and night. At the commencement of the fever his head began to enlarge, the ſutures to be ſeparated, and he never afterwards retained the faculty of ſucking to ſuch a degree as formerly. From that time, his head had always a tendency to fall to the left ſide, and it could not be kept erect without a ſupport.— After he had recovered from the fever, an eruption appeared on his right ſhoulder, and on the left ſide of his breaſt, and began to ſpread by degrees over his body, his brows, and the back of his head. The matter diſcharged by the *ſores* had a ſaltiſh, diſagreeable ſmell, a watery appearance, and ſeemed to be of ſo

infectious

infectious a nature, as to raiſe painful and ſimilar *ſores* on the arms and breaſt of his mother. He was eaſier, and he generally ſlept better, when the *ſores* or *puſtules* diſcharged much.

Thus, I have given as diſtinct an account of the ſymptoms of this extraordinary caſe, prior to the time I was called, as I could learn from the mother and the midwife.—I was ſent for to ſee him when he was about ſeven weeks old; at that time, his eyes were a little turned to the left ſide, and ſeemingly fixed in that ſituation; they were always open, even when he ſlept, yet, they were not more prominent than uſual; but the pupils were conſiderably dilated: light did not in any wiſe affect them. He was ſo acute in hearing, that the leaſt noiſe ſtartled or wakened him. His mouth was very large, and conſtantly open, except when he ſwallowed any thing. His tongue was ſhort, and far back in his mouth: he could never be ſaid to be altogether free from fever, for his pulſe continued ſo quick and irregular, that I could not number it. There was ſcarcely any alteration on him from the ſeventh to the tenth week of his age, except that he became weaker, and his head ſtill larger: he took his bread and milk as he uſed to do. About the end of the tenth week he began to be a little convulſed, on the left ſide in particular: his left arm was turned about in ſuch a manner, as that the palm of the hand was uppermoſt; and the joints of the leg and thigh on the ſame ſide, were ſometimes ſo ſtiff, that they could not be *moved*. There were ſpaſms over his whole body, which went off, and returned alternately. He remained in this ſtate till the thirteenth week; and after that, entirely loſt the faculty of ſucking; but his mother ſupplied that defect, by often milking her breaſts into his mouth. In this ſituation he lived five weeks, then death put a period to his miſerable exiſtence. Before he died he was much reduced, ſo ſtiff and cold, that, had it not been from his breathing, and attempting to cry, and ſometimes a writhing of his body, none could have believed that he was alive: It is ſcarcely poſſible to communicate any adequate idea of ſuch a wretched object. His head, when expoſed to the light, had almoſt, in every direction, a tranſparent appearance, ſuch as a lantern has when covered with oiled paper. During the whole courſe of the diſeaſe, he was perfectly regular in his bowels, and his ſtools were of the natural colour; but within a week of his death they became fœtid, and his belly hard; and a day or two before he died, he made more water than he uſed to do. He continued to take nearly his uſual quantity of food till the night of his death, and even that night, took two or three ſpoonsful of it. He always drank a great deal; ſlept little or none, but cried, or *fretted conſtantly*.

Having

Having obtained leave from his parents to open his head, I called in my neighbour, Dr. Napier, who had ſeen the child when alive, and likewiſe a young gentleman in the town, to ſee it performed. As I wiſhed to preſerve the water carefully, till meaſured, I drew it off with a trocar and canula: after I had drawn off all I could in that manner, I made a longitudinal inciſion in the direction of the ſagittal ſuture, firſt through the ſkin and teguments, and next through the dura mater; and between the dura and pia mater, (where, no doubt, the whole of the water had been lodged) I found a little more, which I likewiſe took out. There were, in all, ſixteen gills of water in the head, and as pure as if taken from a ſpring: the bones of the head were flexible, and ſo ſoft, that I could puncture them with a lancet almoſt in any place. At firſt ſight the ſkull appeared like a ſhell, and not a veſtige of brain to be ſeen, or at leaſt, nothing but empty membranes; but on diſſecting the whole of the membranes, and dividing the medulla oblongata, I found a part of the two poſterior lobes remaining, which were of a ſolid membranous ſubſtance. The left lobe was a little thicker than the right, and of a greeniſh blue. When I cut into it, I found the inſide almoſt of the ſame colour; the right had more of a grey colour. The thickeſt part of them did not exceed three-quarters of an inch, or an inch at moſt. Of the fore part of the brain, nothing remained but a ſhrivelled membrane. All the reſt of the parts had their natural colour. When the membranes, and what remained of the brain, were diſſected, and the veſſels divided that ſupply the brain, &c. there was ſcarcely a drop of blood to be ſeen; and only about ʒj. or ʒij. of bloody water. Although the head had been ſeparated from the body, and immerſed for a conſiderable time in water, it could not be whiter, nor freer of blood than it was. I cannot conceive how the child could live ſo long, with his brain in ſuch a ſtate, and ſuch a preſſure of water upon it; perhaps, a much leſs quantity of water in the ventricles of the brain, would have proved fatal ſooner.

Might not much crying, without a prediſpoſition to the diſeaſe, be the exciting cauſe of hydrocephalus in children? Might not the frequent cleanſing of the ſtomach and the bowels by *purges*, during the firſt three or four weeks of this child's age, have prevented or carried off the diſeaſe?

I am ſorry that the ſhortneſs of the time allowed for the operation, did not permit me to inveſtigate the brain with ſuch exactneſs, as to be able to give you a more diſtinct account of it; but from what I have ſaid, you can ſee that the caſe is a very remarkable one. When you have a leiſure moment, I

ſhall hope to have the pleaſure of your opinion concerning it. I am, with much reſpect,

Dear Sir,

Your's, &c.

Berwic, Oct. 20, 1799. T. GUTHRIE.

To the Editors of the Medical and Phyſical Journal.

Gentlemen,

As the public attention is now much intereſted by Dr. Parry's valuable Treatiſe on Angina Pectoris, an account of a caſe of that diſeaſe, in ſome reſpects remarkable, may not be unacceptable to you. It occurred in the year 1797; but Angina Pectoris is ſo infrequent, that I have had no other opportunity of making trial of the medicine, which ſeemed, in this inſtance, highly beneficial; nor of determining, whether any credit be due to my conjectures on the nature of the diſeaſe. Still, it may not be without uſe, to relate a ſingle inſtance, in which this rare but fatal complaint was removed by means not employed before.

I believe I was not miſtaken, in thinking this caſe an inſtance of the diſeaſe, of which Dr. Heberden gave the firſt deſcription; but that may be determined, and the reaſon for the treatment I adopted may beſt be explained, by tranſcribing part of the notes which I wrote at the time.

Dec. 6, 1797. John Maynard, æt. 61, a carver in wood, was born in Berlin; has lived in London thirty-nine years, and enjoyed extraordinary good health till laſt winter. He is tall, well proportioned, thin, but pretty ſtout for his years, though a little bent forward, probably from ſtooping over his work. His anſwer to every queſtion, though a foreigner, was ſo diſtinct and preciſe, that I could not doubt the accuracy of the account he gave of his illneſs.

"Laſt March he became a patient at the Public Diſpenſary in Carey Street, London, having a cough, ſpitting of blood, and pain in his arms, near the inſertion of the deltoid muſcles, and down his fore arms, in the courſe of the muſculo-cutaneous nerves. He had been ill ſome time, but in about three months he was recovered; he left the Diſpenſary, and had no return of ſpitting of blood."

On the 6th of December he again became a patient at the Diſpenſary, and was committed to my care, by my friend Dr. Willan.

Willan.—"He has ſuffered much for about a month, from fits of "gaſping for breath,"* occaſioned by walking, efforts at ſtool, and bending forwards; but eſpecially by ſtooping to buckle his ſhoes; even very ſlight exertions, as croſſing his legs while ſitting, will bring them on. In theſe fits he has an inexpreſſible ſenſation of ſevere pinching at the pit of his ſtomach, extending upwards, and a gaſping for breath, which oblige him inſtantly to deſiſt from all exertion. On preſſing on the right ſide, below the ribs, near the ſcrobiculus cordis, he feels pain: this is the ſeat of his peculiar ſenſation.† He complains of pain in his loins, and in the iliac regions on bending his body. He has a conſtant cough, with very little expectoration. The peculiar ſenſations juſt deſcribed, have never ſeized him during a fit of coughing, but they have ſometimes followed that effort. He never has had the ſenſation of his heart ceaſing to beat; but on recovering from a fit, he feels it "flutter:"‡ It cannot be felt in any part of the thorax. Since theſe complaints began, he has had no pain in his arms."

"He finds his diſeaſe increaſing: he now is never a whole day without fits, and ſometimes has many in a day; they come on even when he is in bed, but are not more ſevere at night than in the day. He is habitually coſtive, having had a motion of the bowels only about once a week, for ſome years."

"His pulſe was ſlow, and very weak; but ſo little remarkable, that, having felt it for a ſhort time, I did not count it by my watch."

"He has no ſymptoms which indicate with certainty, an organic diſeaſe of the heart; though his age leaves little doubt that the arteries, and perhaps the valves of the heart, are not in their natural ſtate. If there be no other diſeaſe, it is probable that the elaſticity of the aorta is impaired; for this artery becomes leſs and leſs elaſtic, as old age advances. The valves at the origin of the aorta, in conſequence of this change, perform their office imperfectly;‖ ſome part of the blood returns into the heart, and the impulſe of the heart on the blood in circulation is diminiſhed."

"The

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* This is his own expreſſion. I never ſaw him during a paroxyſm.

† Dr. Heberden ſays, the pain always inclines more to the left ſide. See Med. Trans. Vol. II. p. 63.

‡ This word was uſed by the patient; but he did not mean to expreſs by it, a palpitation of the heart;—he never had palpitation of the heart.

‖ Theſe valves not being fully extended, the blood will be impeded in its entrance into the coronary arteries.

"The symptoms of true* Angina Pectoris, in which no change of structure in the heart or its vessels, is discovered in dissection, may, perhaps, depend on defect of force in the heart. It is probable, that in some instances that organ becomes absolutely weaker; as it has been found on dissection, that the fibres had lost their tenacity in an extraordinary degree: In other instances, the muscles may not have lost their vigour; but the obstacles to be overcome may have been increased."

"These conjectures respecting the nature of the disease, seem not improbable; for symptoms resembling Angina Pectoris accompany some organic diseases, in which the heart cannot communicate to the whole mass of blood, an impulse equal to that which it imparts in a sound state."

"In Maynard's case, I suspect that the power of the heart is diminished, as its motion cannot be felt, and the pulse is very feeble; I therefore wish to try the effect of *argentum nitratum*, for that substance has a very remarkable influence both on the nerves and the muscles, even when their communication with the brain is destroyed, and their supply of blood cut off; and it has been, in some instances, an effectual remedy in epilepsy, a convulsive disease subject to irregular paroxysms.—In the year 1793, in making experiments with metals on the irritability of the muscles, both of cold-blooded and warm-blooded animals, I found that the muscles of a detached limb were rendered more vigorous;† and when they had ceased to be convulsed on the application of metals, they were restored to energy by touching a single point of the nerve with *argentum nitratum*. In other experiments, I laid a frog, with its head cut off, and abdomen opened, upon a plate of zinc, and endeavoured to excite convulsions, by making a communication between the viscera and the plate of zinc by a silver probe; but in vain, till I had touched the peritonæum with argentum nitratum. After this application, violent convulsions of the lower limbs were excited, by making a commu-

* This was written under the impression, that the name Angina Pectoris had been applied, in some instances, to diseases which did not correspond with the disease to which Dr. Heberden gave that name, either in symptoms, or appearance on dissection; but in other instances, though the symptoms were the same as those described by Dr. Heberden, the appearance on dissection was very different. He discovered no organic disease. See Med. Trans. Vol. III. p. 8.

† I uniformly found the very opposite effect from *kali purum*; and I in vain endeavoured to restore the energy of the muscles, by the subsequent application of *argentum nitratum*, diluted *sulphuric acid*, and *distilled vinegar*.

a communication again between the viſcera and the metals. Hence it appears, that argentum nitratum applied to a ſingle joint of the nervous ſyſtem, produces ſome change through the whole body of the animal; and that effect ſeems conducive to muſcular vigour."

"I was led by theſe facts, to preſcribe argentum nitratum to Maynard; for it is probable, that taken into the ſtomach, it muſt come in contact with a ſurface, not leſs furniſhed with nerves than the peritonæum. That ſubſtances taken into the ſtomach, produce effect through the medium of the nerves, cannot be doubted, ſince emetics* do not occaſion vomitting when the *par vagum* is divided."

"After what length of time the effect of argentum nitratum ceaſes, I do not know; I never made that an object of *attention* in my experiments; but, from accidental obſervation, I know that its influence is in a ſhort time diminiſhed, and within twenty-four hours has entirely ceaſed: though the ſame effect may again be produced, by again applying the *argentum nitratum* to the nerve. This I have found ſucceed each day for ten days, after the leg of a frog had been detached from the body, from which the head had been previouſly cut off."

"Maynard was ordered to take a quarter of a grain of argentum nitratum, three times a day. He was deſired to uſe aloetic pills, when neceſſary, to regulate the ſtate of his bowels.

"Dec. 8th. The patient ſtanding; his pulſe intermits often, and at unequal intervals: it beats at the rate of 68 in a minute. He is upon the whole, much better. The fits are not ſo eaſily excited, are not ſo frequent, and do not laſt ſo long. Let him continue the argentum nitratum."†

"Dec. 11th. The fits are leſs frequent, and leſs ſevere. His pulſe is about 69: It does not intermit; but, many times in a minute, a beat is perceived much leſs forcible than the reſt."

"Dec. 12th. His pulſe is 67: It does not intermit; but ſome beats are leſs forcible than the reſt: His bowels are regular. On the whole, he is much better."

"Dec. 15th. There is no alteration in his complaints, nor in his pulſe. He was not quite ſo well yeſterday; but he had been coſtive two days, and was relieved after an evacuation: He ſtill complains of pain a little above the left hip, when he bends forwards. He has not quite ſo much pain in his loins."

"Dec.

* See Dr. Haighton's Exper. Med. Soc. Lond. Vol. II.

† This medicine was not omitted till his recovery was eſtabliſhed.

"Dec. 18th. He has been coſtive ſince the 15th. On the 17th, he had pain and fluttering of his heart; to-day he is better. His pulſe is 64; it does not intermit, but its force ſtill remits: He has more pain in his loins. Let him uſe the volatile liniment."

"Dec. 20th. His pulſe is 64, ſtrong, and without remiſſion. He had two fits yeſterday, not occaſioned by any great exertion: his bowels are regular. To-day he had a fit after he reached the Diſpenſary; he had walked more than half a mile. The pain in his loins ſeems moving downward: He had no pinching at the ſcrobiculus cordis, in the fits either of yeſterday or to-day; but he had "fluttering" and pain, beginning about the middle of the ſternum; it moved acroſs the left ſide: the gaſping for breath was not very urgent. Let him take argentum nitratum four times a day."

"Dec. 29th. His pulſe is 68, with one intermiſſion in a minute. He had a ſlight fit on going up ſtairs: He has much leſs pain."

"Jan. 8th. He has pain in his right ſide, and the calves of his legs: the pain is nearly removed from his left hip. He has had no fit ſince the laſt report: His bowels are at preſent coſtive: His pulſe is 64, regular and ſtronger. He looks much better, and fatter in the face."

"Jan. 15th. He is much better; he had a fit on the 13th. His pulſe is 64, and regular."

"Jan. 19th. He has had no fit ſince the 13th: His pulſe is 61, and regular. Let him continue the argentum nitratum; and take a ſcruple of Peruvian bark three times a day."

"Jan. 24th. His pulſe is 69, regular, and firmer, but not full. He has leſs pain in his back: the bark keeps his bowels open. Let the medicines be continued."

"Feb. 9th. He is much better. After this time he had very few, and thoſe only ſlight returns of fits. His urine was never copious."

About the middle of February I left London; but I often heard of Maynard; and in October, 1797, I heard that he had continued free from his former complaints, and was pretty well; but was obliged, occaſionally, to take aloetic pills.

I have tranſcribed each day's report, to ſhew the progreſs of the caſe. The remiſſion of the ſymptoms from the time when he began to take argentum nitratum, though the diſeaſe was daily growing worſe, leaves no doubt that the recovery ſhould be attributed to this medicine: It produced no ſenſible effect; it did not even keep the bowels regular, though I have ſometimes found it occaſion diarrhœa. That is the only effect I ever

ever obſerved from argentum nitratum in many different diſorders, in ſome of which it proved a ſucceſsful remedy.

I have ſtated my conjectures on the nature of the diſeaſe, as they occurred; and I think them corroborated by the facts that Dr. Parry has lately publiſhed. He relates, that in ſeveral inſtances of Angina Pectoris, the coronary arteries, the veſſels that carry the blood for the ſupport of the heart, were oſſified; and in ſome inſtances, their capacity was diminiſhed by a membranous tube, formed within them.* Theſe, or any diſeaſes, which may diminiſh the quantity of arterial blood diſtributed through the heart, will probably impair the muſcular force of that organ; for all other muſcles loſe their power when the ſupply of arterial blood is diminiſhed.

Fourcroy, with ſome of his aſſociates in the Academy of Sciences, proved by accurate experiments, that the quantity of oxygen abſorbed in reſpiration, is increaſed in proportion to the increaſe of muſcular exertion: the frequency of the pulſe is alſo increaſed. And Anatomiſts obſerve, that the quantity of blood diſtributed through muſcles, is in proportion to the exertions to which the muſcles have been accuſtomed. The colour is higher, and the blood veſſels are larger, in thoſe muſcles which have been accuſtomed to much exertion, than in correſponding muſcles in other bodies, which have been leſs exerted. From theſe facts, it is evident, that the blood veſſels do not contain more arterialized blood, than the muſcles require for ordinary exertions; and therefore, when the quantity is diminiſhed, the muſcles muſt be unequal to their higher exertions. The heart, therefore, when its coronary veſſels are diſeaſed, will not be able to propel the blood when returned upon it in greater quantity, from exertions of the body, or affections of the mind; nor when the obſtacles to the motion of the blood are increaſed by the poſture of the body.

From the ſymptoms of Angina Pectoris, and from diſſection, it ſeems probable, that this diſeaſe ariſes from a defect of force of the heart, either abſolute or relative.

The blood, in conſequence of ſuch diſeaſe in the heart, is leſs quickly ſubmitted to the influence of the air in the lungs; hence follows a ſenſe of ſuffocation, which gives riſe to more frequent efforts of breathing, to gaſping, and deep ſighs. And the heart perhaps, from the diſproportioned effort, may

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* See Inquiry into the Symptoms and Cauſes of Syncope Anginoſa, p. 4, 12, 24, 33.

may be ſeized with cramp, as other muſcles are ſeized with cramp in great, and eſpecially in protracted, efforts.

The caſe I have related, leads to many other obſervations; but I have intruded too far on your Journal, to add more than that I am,

GENTLEMEN,

With great reſpect, your obedient ſervant,

York, Feb. 20, 1800. ROBERT CAPPE.

P. S. Theſe notes were ſent to a friend in London, in whoſe judgment I have great confidence, with a requeſt that he would tranſmit them to the Editors of the Medical and Phyſical Journal, if he thought they might be worthy of their attention. My friend expreſſed a doubt, whether the caſe was really Angina Pectoris, and ſuggeſted a change of title for the paper. I ſhould acquieſce in his judgment, and adopt the change, if the caſe had not been conſidered by ſome friends who ſaw it, as Angina Pectoris; and the notes, marked with inverted commas, had not been written at the time on this ſuppoſition. I have, therefore, to requeſt of you, gentlemen, to accept them, ſuch as they are; and I am not without hope, that the caſe, which ſtrongly reſembles Angina Pectoris, may not be found unintereſting by thoſe who would aſſign it ſome other name.

To the Editors of the Medical and Phyſical Journal.

GENTLEMEN,

THE appearances after death obſerved in the following caſe, are, I think, worthy of being preſerved in your valuable publication, particularly as nothing ſimilar is to be found in the writings of De Haen, Morgagni, or Doctor Baillie. I ſhall barely enumerate ſuch particulars of the caſe, as may ſerve to ſhew how eaſily the cauſe and ſeat of the diſeaſe might be miſconceived.

Richard Montjoy, aged 8 years, a thin, ſprightly boy, of a fair complexion, with blue eyes, delicate ſkin, and fair hair, was attacked, in the month of June of the year 1796, with a ſevere pain under the ſtomach. As moſt of the diſeaſes of children, in this as well as in other countries, are aſcribed to worms, ſome *worm-powders* were procured from an apothecary in this city, which operated ſtrongly. No mitigation of the pain

pain having however enſued, I was ſent for, and found the boy apparently in great agony; he complained of excruciating and unremitting pain about the lower part of the ſtomach, ſhooting acroſs the abdomen; his countenance was pale and languid, and his pulſe feeble. The pain was neither increaſed nor diminiſhed on preſſure; and as the bowels were free, there was no reaſon to ſuſpect an inflammation of any part of the inteſtinal canal. Different means were tried, with a view of giving temporary eaſe, but no material relief was experienced from any, except from laudanum in pretty large doſes; he continued ſeveral weeks in this ſtate, taking very little nouriſhment, and complaining of conſtant pain.

On being queſtioned relative to the cauſe of his complaint, he could aſſign none, but recollected having received a ſevere blow of a rattan acroſs the loins from his ſchool-maſter. About the commencement of the following winter, after he had renounced the aid of medicine as fruitleſs, his health began ſuddenly to mend, and he experienced temporary ceſſation of pain. Thoſe ſymptoms became gradually more favourable, until he at length entirely recovered.

From the winter of 1796 until the month of Auguſt 1799, he continued in perfect health. About the end of this month the pain about the ſtomach returned, without any apparent cauſe, and preciſely in the ſame ſituation as before; it was ſlight at firſt, but increaſed daily: His appetite was at firſt very bad, but afterwards became voracious; on taking food, however, the pain was much aggravated, but he felt relief from bending his body forwards. He was often attacked with diarrhœa, of the appearance of blood and pus; at other times the bowels were conſtipated. He was bliſtered, and took a variety of medicines, without any benefit.

Though during the three or four laſt months of his illneſs he took a conſiderable quantity of nouriſhment, he viſibly became more emaciated and feeble. His pulſe was, during this time, about 120. His countenance reſembled that of a perſon in the laſt ſtage of a pulmonary conſumption; it was pale, and the hectic fluſh was diſtinctly marked on each cheek: he had conſtant, profuſe, and univerſal perſpiration; the abſence of cough, however, during his whole illneſs, together with the ſeat of the pain, left me no room to doubt that the lungs were not the ſeat of the diſeaſe.

About three months after the firſt attack, the pain became gradually milder, and his appetite returned; he was, however, ſtill much emaciated, and never totally free from pain. He was bliſtered, and took mercury until the mouth was affected, without any advantage.

He ſpoke of the torments he endured rather in a plaintive than a querulous tone; he bore them, indeed, with a greater degree of reſignation than could be expected at his age. He ſeemed ſtrongly impreſſed with the idea that he would never recover, and frequently told his parents that he knew full well they could form no idea of the degree of his ſufferings.

About the middle of laſt May he expired; a few hours before his death he had eaten ſome veal for dinner.

His parents were anxious that the body ſhould be opened, in order that, if poſſible, the cauſe of his diſeaſe might be diſcovered. I conſequently called on Dr. Bullen, Dr. F. Walſh, and Dr. John Milner Barry, of this city, for that purpoſe.

After making a conical inciſion, and expoſing to view the viſcera of the abdomen, their appearance was quite natural; no omentum could however be obſerved, as it probably had been waſted away during his tedious illneſs: The liver appeared rather prominent, but quite ſound; the ſpleen and ſtomach exhibited no marks of diſeaſe; the inteſtines appeared natural, and no worms were found in their cavity; the meſenteric glands were very little enlarged.

On raiſing up the liver, immediately under it appeared a large, oblong tumour, which at the firſt view appeared to be a conſiderable enlargement of that portion of the colon, which after aſcending above the anterior part of the right kidney, paſſes under the liver and gall-bladder. On tracing, however, the inteſtines from the pylorus to the rectum, the canal was found quite natural, both with reſpect to diameter and appearance. On minute inveſtigation, we found this tumour attached to the right kidney, and extending from it to the diaphragm, to which it adhered by cellular ſubſtance. Having carefully diſſected it out of the abdomen, we ſeparated the kidney, which did not exhibit the leaſt appearance of diſeaſe. The tumour was of an oblong ſhape, about ſix inches long, and from two and a half to three in its largeſt diameter; it was of the ſame colour with the inteſtines externally; it was of a firm conſiſtence, and on cutting into it, we found it compoſed of a yellowiſh-coloured ſubſtance, with a ſhade of brown, not unlike the remains of beef-ſuet, after it has been melted and the tallow ſqueezed out of it. After being ſeparated from the kidney, it weighed $2\frac{3}{4}$ ℔. We had now no heſitation in pronouncing this tumour to be a conſiderable enlargement of the *capſula renalis.*

On raiſing up the ſtomach and ſpleen, a ſimilar tumour was perceived on the left ſide, which on examination was found to be the *capſula renalis* of that ſide alſo conſiderably enlarged; it reſembled in appearance that of the right ſide, but was much

more

more firmly connected with the diaphragm. On one of its surfaces the pancreas was found firmly attached to it, and was thus removed from its transverse situation in the abdomen, by which it is parallel to the stomach, to form an acute angle with that organ. The pancreas was not in the least enlarged, nor scirrhous, as there was some reason to expect. As so firm an adhesion between it and the tumour must have been the consequence of inflammation, to this may be referred the acute pain felt at the commencement of the disease. The capsula renalis of the left side, together with the kidney, which was of the natural size, weighed 3¼ ℔.

On opening the thorax, the lungs were found quite sound, and no marks of scrophula could be discovered.

CHARLES SUGRUE, M.D.

Corke, June 26, 1800.

To the Editors of the Medical and Physical Journal.

——— Exempta juvat spinis e pluribus una. HOR.

GENTLEMEN,

ON perusing the Observations of Mr. DAVID UWINS, in NO. XVI. of your truly valuable Miscellany, I was prompted to enlarge on the paper you honoured me by inserting in the preceding number; but my professional avocations prevented the execution of my intention during the course of the last two months. Leisure now permits me to make a few cursory remarks, which, should you indulge with a place in your Journal, will be thankfully esteemed by me, as an additional favour to those already conferred by your attention.

That gentleman observes, that "he suffered a degree of disappointment in finding no additional matter to what had been before advanced on the subject, excepting the publication of a few cases, which were by no means necessary to strengthen the evidence against all kinds of nostrums and specifics." I cannot, in the first instance, readily conceive the cause of that gentleman's disappointment. He acknowledges, that he observed in the table of contents, a paper "On the pernicious Effects of Quackery." I believe that paper, on perusal, he found correspond with its title. Had the table announced a communication *on the most effectual means of exterminating Quackery*, such an event would have been natural. He proceeds to assert, that those Observations were unnecessary. In

this alſo I cannot acquieſce. Mr. U. previouſly obſerves, that "the ſubject had been for ſome time in a ſtate of dormancy." It follows, therefore, that I was juſtifiable in the attempt at its reſuſcitation. It appears, its revival was neceſſary to call forth the latent opinions of this gentleman on the ſubject, (which were certainly ingenious and judicious). I am, however, inclined to proſecute further a matter of ſo much importance, and probably in a more explanatory manner. I do not, as inſinuated, preſume to aſſert, that a "neglect of legiſlative interference is the baſis of the evil in queſtion: By no means. When I hinted that ſuch an interference was neceſſary, I did not conceive it probable that it could be exerted towards its total ſuppreſſion; whilſt the revenue is benefitted by the advantages accruing from the permiſſion of patent medicines, ſuch an event is ſcarcely to be hoped for. My plan, I flatter myſelf, is not ſo chimerical; its utility I ſubmit to your ſuperior judgment. For mine own part, I do not conceive a total ſubjugation of theſe vaunting empirics neceſſary. If I can contribute to the ſecurity of the welfare of my countrymen, my aim is accompliſhed. — I would have a law enacted, that if any perſon has a recipe for which he wiſhes to procure a patent, (whether his own invention or communicated to him by others) the noſtrum ſhould be firſt of all ſubmitted to the inſpection and analyzation of a committee of profeſſional men, eſtabliſhed for that purpoſe. If, after duly inveſtigating its component parts, they ſhould be of opinion that its adminiſtration would be ſerviceable in any particular complaint, to ſuch diſeaſe alone ſhould it be appropriated; and if, thus authorized, he obtain a patent, he ſhould incur a heavy fine, ſhould he at any time advertize the ſame noſtrum as a remedy in any other diſorder. By theſe means, although from political motives an evil is tolerated, it will be diſarmed of its power to do miſchief.

Mr. Custance obſerves, (in No. XVII.) that caſes deſigned to evince the dangerous effects of Quackery to its deluded followers, ſhould be communicated to the public through the ſame channel as the doctors' advertiſements. He will permit me here to diſagree with him. So marked an attention to the machinations of theſe worthleſs miſcreants, would be too great a condeſcenſion on the part of the Faculty, and would ultimately conduce to their advantage; for theſe wily peſts would find little difficulty in perſuading their credulous victims, that the ſuper-eminence of their characters, and the aſtoniſhing efficacy of their medicines, had attracted the envy of the profeſſion, who were obliged to adopt that method to depreciate its value. Theſe inſinuations would be readily ſwallowed by ſuch perſons as are uſually operated on by advertiſements of that nature.

Thus,

Thus, in attempting to leſſen their credit, we ſhould, on the contrary, ſtrengthen their power, and furniſh them with materials to purſue their deſtructive career.

I ſhall not proſecute this ſubject further at preſent, but hope that thoſe gentlemen who have already given their opinions on the ſubject will with freedom reject, improve, or enlarge on the plan I have hinted thus lightly, or ſuggeſt other means more prudent or practicable.

I am induced from lately obſerving the frequent diſputes among ſeveral of your correſpondents, reſpecting the extraction of the placenta, (without hazarding an opinion myſelf) to ſtate two caſes which occurred to me not long ſince.

On Sunday, April 20, I was called to Elizabeth Baker, of Weſtwood, a ſmall village near this town, to attend in delivery. When I arrived, I found her ſtanding with her back reſting againſt the wall; and learned from the attendant women, on enquiry, that ſhe had been in this erect poſition for ſome hours. Not being able to ſit or even lie down, without the greateſt inconvenience, I concluded at the moment, from this information, that delivery was very near; I therefore placed her gently on the bed, and proceeded to examination. On attempting to introduce my fingers, I felt ſome reſiſtance; and examining further, I found that the uterus and its contents were ſo far preſſed into the vagina, that ſome part protruded a little through the os externum; a pain which immediately followed, would have forcibly expelled it, had not I made a conſiderable reſiſtance with my hand. The os tincæ not being dilated to the compaſs of a ſhilling, I was convinced that delivery was for the preſent impracticable; I therefore attempted to return the uterus, and happily ſucceeded; and a copious evacuation of urine ſhortly ſupervened, which diſcharge had been ſuppreſſed nearly two days by the preſſure on the meatus urinarius. I directed that ſhe might be conſtantly kept in a horizontal poſition, and the bowels moderately open by occaſional emollient clyſters, an anodyne being now and then introduced. She thus continued a week without a recurrence of pain, prolapſe of the uterus, or impediment in the alvine or urinary evacuations.

On Monday the 28th, I was again called, about one o'clock in the morning. On arriving, I found the child had been delivered without aſſiſtance, about three-quarters of an hour; that conſiderable hæmorrhage had enſued, but had then ceaſed; and the placenta was not yet extracted. The woman being extremely weak, and conſiderably exhauſted by the effuſion of blood, I thought proper, as the hæmorrhage had ſubſided, to wait

wait some time before I attempted its extraction: But some slight pains occurring, (which I hoped would effect its expulsion without difficulty) I made some efforts to bring it away, by gently drawing the funis; but in vain. On introducing my hand, I found a morbid adhesion had taken place near the fundus uteri. As the uterus had not much contracted, I desisted from further endeavours, considering the weakness of the patient, whose situation was rather perilous from recent exhaustion; her lips livid, pulse small, and her whole countenance pale and ghastly. I determined to leave its expulsion to the efforts of Nature, and waited with patience about four hours, contenting myself with gently straining the funis at the recurrence of every pain. By this time the patient became very importunate in her request to have it taken away at all events, being extremely anxious to be put into bed; I therefore, in compliance, again introduced my hand, and removed several large clots of blood which obstructed its passage; then cautiously introducing my fingers round the whole substance of the placenta, between it and the uterus, I gradually detached and brought it away entire. Slight hæmorrhage succeeded, and repeated faintings, which were so alarming that I began to wish I had persisted in my original resolution of trusting the operation to the efforts of Nature, which I doubt not would have been sufficient, had I not acceded to her impatience. By administering cordial, tonic, and volatile draughts frequently, she soon recovered, and was, in the course of a fortnight, able to leave her room without material injury or inconvenience.

The second case is of Elizabeth Munday, residing at Avoncliff, near this town. I was called on to attend her in labour, May 16. The presentation of the child was natural, and nothing material occurred prior to delivery, which happened soon after my arrival. I did not intend to proceed to the extraction of the placenta until Nature evinced by her efforts for its expulsion, that my assistance was necessary. But about ten minutes after the birth of the child, a hæmorrhage ensued, which in a short time became so profuse as to greatly endanger the life of my patient. I then judged it expedient to attempt its delivery without further delay. I took hold of the funis, which was of a texture uncommonly slight, and drew it in the gentlest manner; but unfortunately, notwithstanding I proceeded with the utmost caution, it separated at the placenta. Although such an accident was new to me, I did not suffer alarm to conquer my circumspection. I cautiously introduced my hand into the uterus; and, as in the preceding case, gradually detached the placenta, which I brought away entire. Hæmorrhage still continued, and I became extremely apprehensive for my patient's

tient's ſafety; but happily, by the application of wet cloths, &c. and ſupporting her occaſionally with cordials, I ſucceeded in removing every alarming ſymptom, and ſhe is now tolerably ſtrong and healthy.

The former of theſe caſes may tend to exemplify, that the placenta may be retained with ſafety a much longer time than is generally admitted; and the latter, that although a forcible extraction is ſometimes neceſſary from accident, it may be eaſily effected without the neceſſity of thruſting the fingers into the ſubſtance of the placenta.

Before I conclude, I ſhall further intrude on your patience by calling your attention to another ſubject. In the firſt Vol. of your Miſcellany, p. 181, Profeſſor Hufeland, of Jena, recommends the uſe of artificial muſk in the hooping cough. I have made ſeveral eſſays of the virtues of that compoſition, and find it efficacious not only in that complaint but in ſeveral others, a few of which I ſhall briefly ſtate to you. I generally adminiſter it in the form of a tincture, thus prepared: Two drachms of the reſinous extract, obtained by the proceſs there recommended, I diſſolved in eight ounces of alcohol, which formed a tincture of a paliſh yellow colour, and afforded a very grateful ſmell. Two ſons of a gentleman at Briſtol, the one about nine years of age, and the other ſix, were afflicted with the hooping cough. In the eldeſt, an epiſtaxis conſtantly attended the paroxyſms of coughing, accompanied with ſevere pains in the region of the thorax. The diſorder in both ſeemed to reſiſt the power of the remedies uſed. Emetics, &c. were adminiſtered by a medical gentleman there reſident, but without any viſible good effect. Having buſineſs that called me to that city, I accidentally ſaw the children, and adviſed their friends to make trial of this ſpecific, which they agreed to. I ſent a phial, containing about ℥iſs, directing the eldeſt to take twelve and the youngeſt ten drops of the tincture three times a day, in a tea-cup full of barley-water. I have, a few days ſince, received an account of its good effects, in a letter from my friend, a ſhort extract from which I ſhall here tranſcribe. "The bottle of drops you ſent me for John, has perfectly cured him of the hooping cough; on taking it, he immediately found relief, as it removed the pains he complained of about his cheſt, and ſtopped the bleeding of his noſe. Theſe drops, without the aid of any other medicine, have perfectly reſtored him. The youngeſt is alſo nearly recovered."——In two or three other inſtances I have alſo witneſſed its good effects; and am of opinion, that in this complaint its efficacy ſtands unrivalled.

In

In a caſe of Diabetes Mellitus, its adminiſtration was attended with ſingular advantage. It occurred in an elderly man, who reſides at Broughton, near Melkſham, in this county.—The concomitant ſymptoms ſufficiently characteriſed the diſeaſe: Great emaciation, loſs of ſtrength and appetite, with a ſlight degree of hectic. The diſcharge of urine was very conſiderable, and varying in its appearance, ſometimes aſſuming that of whey, at other times (except when held in a glaſs to the light) perfectly limpid; the taſte and ſmell were ſuch as are commonly diſtinguiſhable in that diſorder. I gave him a conſiderable quantity of hepatiſed ammonia, recommended alkaline drinks, and a total abſtinence from vegetable food and fermented liquors. The diſeaſe did not, however, ſeem diſpoſed to yield to this mode of treatment; on the contrary, the ſymptoms became daily more formidable. Conſidering his caſe hopeleſs, and, I confeſs, with no very ſanguine expectations of ſucceſs, I determined on a trial of the tincture before deſcribed. I directed him to take 25 drops bis terve die, perſiſting in the ſame regimen I before adviſed, and omitting the hepatiſed ammonia. I was agreeably ſurpriſed by his ſpeedy amendment, and at length his complete recovery. From the time he commenced taking the drops, the alarming ſymptoms gradually ſubſided, his appetite returned, his ſtrength became by degrees repaired, the urinary evacuations became leſs frequent, never involuntary, diveſted of its ſaccharine taſte and peculiar ſmell, and its appearance healthy; in fine, he ſoon found himſelf in a ſtate of complete convaleſcence. I have found it a powerful tonic and corroborant in nervous diſorders, and caſes of extreme debility; ſeveral of which, would time permit, I could now lay before you. This, however, I muſt defer till a future opportunity; I fear I have already treſpaſſed too long on your patience, for which I ſolicit your excuſe. Accept my warmeſt wiſhes for the proſperity of your excellent and laudable undertaking; and believe me to be, with reſpect,

GENTLEMEN,

Your moſt obedient ſervant,

Bradford, Aug. 9, 1800. O. W. BARTLEY, Surgeon.

DR.

Dr. MACLEAN, on DIGITALIS.

[Continued from pp. 127—141 of our laſt.]

My ſtrongeſt and moſt vigorous plants grow in a bed or bank ſloping to the ſouth, at the diſtance of ſeveral feet from each other, in an oval, raiſed about a foot and a half above the level of the ſurrounding ground. This was a piece of ground originally detached from the kitchen garden, and raiſed about two feet above its level, (whether by nature or art I know not,) allotted for the growth of ornamental flowers and ſhrubs. The ſoil was naturally light and dry, and it was rendered ſtill more ſo by the addition of a portion of coarſe gravel. But on obſerving the objections urged againſt Horticulture as a general principle, I was deſirous of aſcertaining how far they might be well founded. With this view I directed my gardener, laſt autumn, to tranſplant about 100 plants into the rich ſoil of the kitchen garden, and to add a quantity of gravel ſufficient to looſen its texture. They furniſhed me this ſummer with an abundant crop of leaves, equally ſtrong, healthy, and vigorous, with any I had gathered, although the higheſt ſtems did not exceed four feet. The principal circumſtances I find neceſſary to its ſucceſsful culture in a garden, are, a light dry ſoil, and a ſituation freely expoſed to the ſun and air. The roots of the Fox-glove are extremely ſmall and numerous, and ſeem incapable of penetrating through a denſe ſoil.

The Doctor endeavours to impreſs the belief, however, that he has the authority of eminent botanical writers, from the fifteenth century to the preſent day, in his favour. After repeating his favourite epithets, "low and *damp* ſoil, or a *chalky* ſoil, or a *fat* and *rich* ſoil," which he tells me, "if I conſult the writers upon the natural hiſtory of the plant, will diſcover, are highly detrimental to its growth;" as if I had inſiſted upon theſe being abſolutely neceſſary to it; he obſerves, "that Dr. M. in inſiſting on free expoſure to the air, &c. is in another inſtance deviating widely from the known habitudes of the wild plant;—that botanical writers would inform him, that ſheltered ground is the favourite ſituation of the Digitalis, which courts ſhade, is *always* found in deep hedge-rows," &c. "That I may not be taxed, however, with general or unauthoriſed aſſertion," continues he, "I ſhall conclude theſe remarks by quoting ſome of the firſt, both old and modern writers, on the natural hiſtory of the plant."

"Naſcitur in *montibus*, *umbroſis* et *ſaxoſis* locis. Fuſchius."

"Fox-glove groweth in *barren*, *ſandy* grounds, and under hedges. Gerard."

"It grows only in *gravelly* ſoils, *rarely* or *never* on thoſe where there are *ſtrata* of *calcarious* earths. Lewis."

"Digitalis. Place; *dry*, *gravelly*, or *ſandy* ſoils;" *particularly on ſloping ground.** "Withering."

Now I muſt take the liberty of telling the Doctor, that none of theſe authors, nor any that have fallen within my reach, appear to me to have advanced any thing tending to eſtabliſh what he has aſſerted.

If Fuſchius ſays, "Naſcitur in *umbroſis*," he likewiſe adds, "*montibus* et *ſaxoſis locis*."

If Gerard tells us, "it groweth under *hedges*," he alſo mentions, "in *barren*, *ſandy* grounds;" Lewes and Withering ſay nothing of *ſhade*; and the ſoils and ſituations deſcribed by them, as friendly to its growth, are in general, the reverſe of *ſhaded*, dry, and gravelly ſoils; and ſloping ground, being, I believe, either elevated or expoſed. I can diſcover nothing like, "*always found in deep hedge-rows.*"

If in hedge-rows, on the ſides of banks, and *particularly on ſloping ground*, the Digitalis be ſhaded on one ſide from the ſun, it receives his perpendicular rays on the other; if it be ſheltered on one hand from certain winds, it will be fully expoſed on the other.

It appears evident, therefore, that in regard to ſhade, even from the authorities he has himſelf cited, the balance is conſiderably againſt him.

Nor does he ſeem more fortunate in adducing their teſtimony, as adverſe to its culture in a garden. Like all other botanical writers on the natural hiſtory of plants, they ſpecify the ſoils, ſituations, and habits congenial to them; but does it thence follow that they meant to deny them a place in the garden? If this were the caſe, many hundreds of plants now in high cultivation in gardens, muſt neceſſarily be excluded. The ſoils mentioned by theſe reſpectable authors, may as readily be imitated in the generality of other gardens, as they have been in mine, as far at leaſt as they may be thought neceſſary. In ſpeaking of the common garden Digitalis, it appears to me, that Dr. D. judges of its virtues entirely from its external appearance, without having put them to the teſt of experiment; a mode of inveſtigating truth which muſt often lead to error. There is an obvious difference in the ſize and heighth of theſe and the wild plants; yet not ſo great a variation in their internal qualities. I have had frequent opportunity of making a compariſon, not only between the leaves of two pariſhes

* Why Dr. Drake ſhould have omitted this part of Withering's hiſtory, is beſt known to himſelf.

riſhes contiguous to that in which the Doctor and his friends loaded their cart, but thoſe of ſeveral gardens, in which no attention was paid to the culture of the plant, more eſpecially thoſe of the late Mr. Downes, ſurgeon, of this place, on which my firſt trials were made; of Mrs. Young, and S. Briſe, Eſq. both of Clare; (Dr. D. obliges me to mention names and places.) The firſt was not inferior to any I have ever uſed; and an epileptic boy ſeemed as readily affected by the tincture made from thoſe of the latter, as by any other; nor did the medical men among whom it was diſtributed, complain of its want of power. The trials, however, were not ſufficiently numerous, nor made with that preciſion and accuracy neceſſary to form a correct judgement of their relative ſtrength. One thing, however, I will venture to affirm, that the common garden Digitalis is much more to be relied on than that which is indiſcriminately gathered, and in general found in the ſhops.

I wiſh it to be underſtood, that I have entered on this diſcuſſion concerning the culture of the plant, ſolely with the view of ſhewing the weakneſs and the fallacy of the arguments adduced againſt it; for if the authors quoted by Dr. D. and all who have writen on its natural hiſtory, had poſitively affirmed that it would not thrive in a garden, I ſhould feel myſelf under the painful neceſſity of contradicting them.

It gives me pleaſure to obſerve, that from the attention now paid to its preparation and culture, the medical world are likely to be in poſſeſſion of the genuine leaves, which alone can enſure ſucceſs; for no plant with which I am acquainted, requires more care in the preparation, or attention in the exhibition, than the Digitalis; yet it ſhould be added, that none rewards the practitioner more for his pains.

In the analyſis of my remarks, Dr. D. thus proceeds: " The inferences which I have drawn from ſome well-known facts in Phyſiology, and to which I am indebted for the uſe of the Digitalis in conſumption, appear to me to have been miſunderſtood by Dr. M. and to have been objected to on inſufficient grounds. In the preſent ſtate of medical ſcience, no one who has duly cultivated it, and who wiſhes to diſtinguiſh his practice from empiriciſm, can or ought to exhibit a new and powerful agent without founding ſuch exhibition on the deductions of ſcience and analogy." And farther,

" *There are ſome modern practitioners, obſerves Dr. Darwin, who declaim againſt medical theories in general, not conſidering that to think is to theorize; and that no one can direct a method of cure without thinking, that is, without theorizing: and happy, therefore, is the patient, whoſe phyſician poſſeſſes the beſt theory.*

" Impreſſed with the truth of this obſervation, I ſhall attempt

to prove, and it will be no difficult taſk I think, that what has been termed '*a theory totally inadmiſſible*,' is but, in ſhort, a ſtatement of facts, drawn from broad experience, and that the only part which merits the appellation of theory, is a mere analogical deduction ariſing from theſe facts. In order to render this more clear, I ſhall ſtate, in numerical order, the different data. 1ſt. Pus is a ſecreted fluid, the conſequence of certain diſeaſed motions of the extremities of the blood veſſels. 2. Hectic fever ariſes only from the matter of an open ulcer. 3. What is termed laudable pus when ſecluded from the air, is neither capable of creating fever, nor, except by its gravity, can it irritate the parts on which it reſts. 4. When pus is excluded from atmoſpheric air, it rapidly attracts oxygen, and an acid of a peculiar kind is generated. 5. Hectic fever is the effect of the abſorption of aerated matter." (Ib. vol. ii. p. 422.) He then proceeds to ſtate the grounds on which theſe data are founded; " and ſhould it be made to appear," ſays he, " that there is juſt grounds for thinking theſe affirmations to be matters of fact, no one will probably deny that the curative proceſſes are legitimately deduced." (Ib. p. 423.)

That I may have unfortunately miſunderſtood the Doctor, " and objected on inſufficient grounds to what he terms, 'well known facts,' is not improbable. Different minds, and ſometimes happily for mankind, form different conceptions of the ſame things and objects. But whether I have done this, " without thinking, that is, without theorizing," is not for me to determine. If I may be allowed this privilege, I own they never ſtruck, nor do they now ſtrike my mind, as laying any claim to the dignified appellation of *facts*, but to be mere phantoms, not of *his*, but chiefly of Dr. Darwin's imagination; which, when we attempt to aſcertain their reality, elude our graſp, and vaniſh like a ſhadow. If this ſhould be proved therefore, it is not likely I ſhould admit, " that the curative proceſſes are legitimately deduced."

As I purpoſe, on a future occaſion, to take a comprehenſive view of this hypotheſis, (which I ſhall ſtill continue to term, " a theory totally inadmiſſible) I ſhall at preſent only offer a few remarks, and contraſt theſe data with what appear to me to approach nearer to facts.

1ſt datum. Pus is a ſecreted fluid, the conſequence of certain *healthy actions** of the extremities of the veſſels which ſecrete it.

2. The

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* The learned phyſiologiſt may, at firſt ſight, be ſurpriſed at the term *healthy* being applied to veſſels in a highly morbid or inflamed ſtate. In an Eſſay

2. The matter of an open ulcer is *not* necessary to the production of hectic fever.*

3. What is termed laudable pus, either when *secluded from* or *exposed to* the air, is neither capable of creating fever, nor, except by its gravity, can it irritate the parts on which it rests.

4. Although the truth of this datum may be questioned, at least when applied to good, thick, bland pus, as found on the surface of a healthy wound or ulcer, I shall, at present, only object to the inference deduced from it; and say that,

5. Hectic fever is *never* the consequence of the absorption of healthy pus; and it might be added, either in its simple or *aerated* state.

The proofs in support of these data shall be given hereafter, when this subject is entered upon more in detail.

Were I to comment upon the above edifying transcript from the

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Essay I have had for some years, in manuscript, on Inflammation, nearly ready for the press, but which has been subjected only to the inspection of a few medical friends who have intreated its publication, are words to the following effect, which, as applicable to the present subject, I shall transcribe: "There are two states or conditions of vessels in inflammation unfavorable for the secretion of good pus; the one *excessive*, the other *defective* excitement, which may be termed *morbid* actions. The interference of art will be necessary in the one, to abstract stimulus and tone; in the other, to add both, in order to bring the vessels to the intermediate state, between these two extremes, which alone is friendly to the formation of good bland matter. This happy medium I call *healthy*, because it is immediately connected with, and absolutely necessary to, a *healthy* or healing process." A few examples in illustration of this are afterwards given, which shall, at present, be omitted.

This state corresponds with what Mr. Hunter termed *healthy* inflammation.

It really appears as inconsistent with the known laws of the animal œconomy, as it is with the rules of sound logic, to call that action *diseased*, which it is the endeavour of every good surgeon to establish, from a conviction that without it he will attempt in vain to bring about the healing process.

* Dr. D. seems rather unlucky in adducing Dr. Cullen's testimony in support of this *datum*, as laid down by him. Even from the passage he quotes from his inimitable works, it does not seem clear that the words, "*purulency* or *ulceration* of some *internal* or *external* part," are necessarily and unavoidably to be interpreted into "an open ulcer." But if any doubt should arise, it will be completely removed on perusal of the following passage, a few pages farther of the First Lines: "A phthisis from a suppuration in consequence of pneumonic inflammation, is that which most rarely occurs in this climate; and a phthisis does not always follow such suppuration, when the abscess soon breaks, and discharges a laudable pus; but if the abscess continue long shut, and *till after a considerable degree of hectic has been formed*, a phthisis is then produced, equally dangerous as that from other causes. (p. 400).

Nor have I been able to discover any thing in Home's excellent Dissertation on Pus, that bears for his argument, although he quotes it likewise in his favour.

Brugman's Dissertation I have not perused.

the Zoonomia, I ſhould be tempted to ſay, there are ſome modern practitioners who declaim againſt experience founded upon experiment and attentive obſervation, and without "thinking, that is, without theorizing, for themſelves, implicitly adopt, without analyſis, every new theory that ingenuity may invent, eſpecially if ſanctioned by the authority of a great name, although it may have no other merit than the charms of novelty to recommend it; and, acting under its influence, they ſeize with avidity upon ſome new remedy, which, from one or two trials, if they chance to be ſuccesſful, they uſher into the world with a character little ſhort of infallibility, and are aſtoniſhed, and not a little piqued, that theſe *unthinking* modern practitioners do not confirm in direct contradiction to common ſenſe, reaſon, and the deliberate reſults of a more enlarged experience:—Unfortunate is the patient who falls into ſuch hands.

No ſuccesſful attempt has yet been made to explain the operation of this plant on the human body: the conjectures hitherto offered, appear in many reſpects vague and contradictory.

If by leſſening the ſuſceptibility of the heart to the ſtimulus of the blood, its contractions be rendered leſs frequent, and the circulation of the blood thus proportionably retarded, it muſt neceſſarily be a direct ſedative to the heart and arteries. Hence, it was employed with this view, and with great ſucceſs, by Dr. Ferriar, in reſtraining inordinate arterial action in active hæmorrhage.

But if, on the other hand, ſerous accumulations be ſpeedily evacuated by its means in dropſy, it will, by parity of reaſoning, be contended, that it acts by a direct ſtimulant operation on the lymphatics and emulgent veſſels; conſequently, that it poſſeſſes directly ſedative and ſtimulant effects on the body at the ſame time. In this manner has its ſalutary agency in the cure of conſumption been explained; and this idea has been carried ſo far, that it is imagined, the action of the abſorbents is ſo powerfully excited, as not only to take up the pus as ſoon as ſecreted, but even the tubercles themſelves.

Reaſoning thus, however, muſt ever lead to endleſs difficulty and embarraſſment. It is, I preſume, as contradictory to the principles of ſound phyloſophizing, as to the known laws of the animal machine, to aſſume it as a fact, that any ſubſtance is capable of producing two directly oppoſite effects at the ſame time on different parts of the ſame ſyſtem of veſſels. Whatever may be its mode of action, it muſt neceſſarily be the ſame, under ſimilar circumſtances, on all parts; the heart, vaſcular, and abſorbent ſyſtem. It is, by arguing in this manner, that there has been ſuch a contrariety of opinion on the *modus operandi* of opium; ſome regarding it as a ſedative, others

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as a ſtimulant, according as they took a more or leſs comprehenſive view of its effects on the body.

On attentively obſerving the action of the Digitalis, it ſtruck me as a ſingular circumſtance, that in all the caſes in which it was preſcribed, (now pretty numerous) unaccompanied with ſigns of ſerous effuſion, I could diſcover no ſenſible increaſe of the urinary ſecretion, nor any of the effects conſequent on the long continued uſe of large doſes of other diuretic medicines, even when the habit had been for ſome weeks under its full influence, although the colour ſeemed at times materially changed: Hence I inferred, that its operation as a diuretic, was immediately and neceſſarily connected with watery accumulation. But, on reflecting that the generality of diuretics increaſe the flow of urine, more or leſs, in health as in diſeaſe, moſt probably by a direct ſtimulant operation on the kidneys, I was unavoidably led to conclude, that the Digitalis poſſeſſed no ſuch effect; and that, as a ſucceſsful remedy in dropſy, its action was chiefly, if not ſolely, confined to the abſorbents. This opinion is ſtrengthened, if not confirmed, by the complete ſucceſs which followed its uſe in two caſes of hæmaturia, attended with dyſuria, and extremely painful micturition, in which I preſcribed it. And, if well founded, of which I have little doubt, it overturns Dr. Ferriar's notion, that "we are debarred from its uſe in one caſe of local increaſed action, that of diabetes." This ingenious author acknowledges his inability to explain theſe *apparently* oppoſite effects, and leaves the queſtion, *ſub judice*; for, ſays he, "I feel it impoſſible to explain this phenomenon at preſent." (p. 13.) And again: "This double effect of Fox-glove, however perplexing in theory, is extremely beneficial in practice; when it takes place, it adds, in hæmorrhages, a mode of natural evacuation, ſufficiently efficacious to relieve plethora, without directly debilitating the ſyſtem." (p. 15.)

But if the view in which I have conſidered this part of the ſubject ſhould be correct, the difficulty is completely removed. It cannot be denied, that by its uſe the lymphatics are placed in a condition of taking up ſpeedily from cavities, inordinate quantities of effuſed fluid; yet, I truſt, I ſhall advance ſome ſteps in proving, that this is not by a ſtimulant operation; indeed, if its ſedative effects on the heart and arteries be admitted, this idea muſt neceſſarily be rejected. The manner in which the functions of the abſorbents are impaired in dropſy, and ſome other diſeaſes, ſeems as yet very imperfectly known; whether by paralyſis, inverted or retrograde motion, or the like, has not been ſhewn; but in whatever way this may be, the

the Digitalis poſſeſſes, in an eminent degree, the power of reſtoring them.

I ſhall be happy in finding my ſentiments, in this reſpect, coincide with thoſe of Dr. Kinglake. In one of his ingenious Communications he obſerves, "My opinion of the *modus operandi* of Digitalis is founded on a principle of lymphatic circulation, that wholly rejects the prevailing but gratuitous notion of the lymphatic veſſels, abſorbing and propelling by virtue of any independently active or contractile power, &c." (*Ib. vol. iii. p.* 125.) Yet, I ſee difficulty in reconciling a direct ſedative operation, which I ſuppoſe this herb univerſally to poſſeſs on the body, with the fundamental principles of the Brunonian doctrine, which he has with ſo much ability ſupported. Permit me in this place to obſerve, in reply to his very candid and liberal remarks on my former paper, that nauſea has, with me, never interfered with the ſalutary agency of Digitalis in conſumption. In nearly all the caſes in which it ſucceeded, (now a conſiderable number) the ſtomach has been more or leſs affected; but when this organ, and the general habit were readily diſordered, its ſucceſs was very uncertain.

I wiſh, in a particular manner, to draw the attention of the medical world to this univerſally ſuppoſed action of the Foxglove on the kidneys; as on the point of view in which I have been led to conſider the ſubject, are founded the moſt important practical inferences; eſpecially in dropſy, which ſhall hereafter be conſidered in detail. With few exceptions, I obſerve the reports of others, who have uſed it in caſes unattended with watery accumulations, accord with my own obſervations. Some ſay nothing of its diuretic effects, while others poſitively affirm, that no ſuch were obſerved. But, as many caſes of conſumption are conjoined with anaſarcous ſwellings, or water in the cavities of the cheſt, the practitioner may, in this reſpect, be ſometimes deceived. "I ſhall have occaſion," ſays Dr. Ferriar, "to mention ſome caſes, in which theſe two operations were combined in a remarkable degree." But I have in vain looked for theſe caſes in his laſt Eſſay.

I am happy in having it in my power to adduce an obſervation of Dr. Drake, in ſupport of the opinion I have been advancing. In endeavouring to maintain the ſuperiority of the tincture over the powder in conſumption, he ſays, "a material difference too, may be obſerved in their mode of operation; for whilſt the powder uſually increaſes, and changes in point of colour, the evacuation of urine, I have *never*, but in one inſtance, and that but in a ſlight degree, perceived a ſimilar effect from the uſe of the tincture." If he had *never* uſed the powder in conſumption, how could he obſerve any difference

difference in their mode of operation? And if he judged only of its effects in dropsy, the difference he mentions was a natural and a necessary consequence.

I venture now, from ample experience, and without the fear of contradiction, to affirm, that no such difference will be perceived by an unbiassed mind, in their mode of operation, in consumption.

I shall take my leave of the Doctor and the subject of the Digitalis, for the present, by assuring him, that, as the investigation of truth, the advancement of medical science, the lessening the sum of human misery, are, and have been, my sole objects in the present and in former discussions; however averse I may be to controversy of every kind, and unequal as I am to the task, when my humble talents are opposed to those of the author of "Literary Hours," I shall not shrink from any farther enquiry that may tend to promote these important ends.

July 1, 1800.

P. S. Since writing the above, while attending with Mr. Travis, surgeon, of East Bergholt, at Higham, one of the parishes which supplied me liberally with Digitalis last year, I was desirous of seeing the plant with this gentleman, more especially as he assisted, about a week before, as well as last year, in collecting the leaves with Dr. Drake. We were conducted to an eminence about two miles from the house where we were attending, and as many from East Bergholt. On the sides of several sandy barren hills, and in adjoining hedge-rows, we perceived some hundreds of plants, a few of which seemed taller than any I had ever seen; but so scantily supplied with thin, narrow, meager leaves, that a boy who accompanied us was some hours in filling a small basket. Mr. Travis informed me, however, they were inferior in every respect to those of his gathering, particularly some he and the Doctor found in a *shaded* lane. When dried, they were shrivelled and discoloured, and apparently far inferior to those of the two gardens already mentioned, as well as that of R. Andrews, Esq. Receiver General for the county of Essex, on which some experiments shall soon be made. Were I to trust to these, I am persuaded I should be disappointed. In hot, dry summers, like the present, when every vegetable growing on sandy soils and elevated situations is soon dried up, it is probable, the Digitalis may be found in higher perfection in *shaded* lanes, where it has more moisture, and is defended from the scorching rays of the sun.

I have lately sent two quarts of the tincture, and a few ounces of the dried leaves of my own growth, to Apothecaries

Hall, as ſpecimens to be referred to, and uſed by ſuch of the Faculty in town as may be intereſted in the ſucceſs of this plant; and may wiſh to ſee and make experiments upon the genuine preparation.

The mixture is made in the proportion of one ounce to eight; and I beg again to obſerve, that this quantity furniſhes more of the active parts of the leaf, than the ſpirit is capable of taking up and retaining; after ſome weeks digeſtion, a conſiderable portion of extract remaining behind on the filtering paper. The ſubject is now, however, in much better hands. In a letter, the learned Preſident of the College has done me the honour of writing me lately, after politely ſaying, "that I deſerve the thanks of the Faculty, for the attention I have paid to a drug of great efficacy;" adds, "that he will loſe no opportunity of mentioning my improvements to the gentlemen of the college, and of trying the effects of my tincture."

Chemical Experiments and Observations, on the Extraction of Sugar and Syrup from Indigenous Plants. By SIGISM. HENRY HERMBSTAEDT.

FROM the chemical analyſis of vegetable ſubſtances, and the knowledge of their conſtituent and other particles contained and mixed with them, it is ſufficiently evident, that the Eaſt and Weſt Indies are not the only countries provided by Nature with ſaccharine plants, but ſaccharine matter is abundantly found in other productions of the vegetable kingdom; and it only requires an aſſiduous examination to point out thoſe vegetables, from which it may be moſt copiouſly, and in the leaſt expenſive way, obtained.

Among the plants hitherto examined, none deſerve to be ranked ſo near the true ſugar-cane, as the whole genus of maple trees; and, particularly, the ſugar and ſilver maple, *Acer ſaccharinum*, and *A. daſycarpon ehrh.* Both trees have been uſed for theſe fifty years to obtain ſugar from,* which in the laſt eight years has proved to be exceedingly profitable.† By my

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* An account of a ſort of ſugar made of the juice of maple in Canada; Philoſophical Tranſactions, No. 171. Klam, in the Tranſactions of the Royal Swediſh Academy of Sciences, year 1751. Memoires ſur les ſucre d'Erable uſité dans le Canada, &c. Hiſt. 1757.

† Notices ſur l'Erable á ſucre des états unis, et ſur les moyens d'en extraire le ſucre, &c. par M. Ruſh, in Rozier's Obſervations ſur la Phyſique, &c. Tom. xli. Paris, 1792.

my own experiments, which I have repeatedly made ſince the winter of 1796, I found out, that from all ſpecies of maples, ſugar may be with more or leſs profit obtained, and that the ſugar and ſilver maple growing even in Germany, though not in the beſt ſoil, give a very good raw ſugar, not inferior to the beſt Weſt India cane-ſugar, and which is got ſo cheap, that a pound of it will come no higher than eighteen or twenty plennige, or about two-pence halfpenny; and only a groſhen, or a penny, when, inſtead of charcoal, common coal or turf are employed for boiling the juice, and particularly when the operation is made upon a large ſcale, as one labourer is able to attend 500 trees during the period of tapping them. The proceſs of boiling the juice is, beſides, ſo very ſimple, that any perſon may ſoon learn it. But theſe advantages are only to be expected from the ſugar and ſilver maple, as the other ſpecies, *Acer negundo*, *A. campeſtre*, *A. platanoides*, and A. pſeudoplatanus contain a leſs quantity of ſugar, which is alſo not ſo rich in ſaccharine matter. However, as plantations of thoſe maples require a ſpace of twenty or twenty-five years before the trees are large enough to admit tapping, it will not be improper, but of great utility to the community, to examine in the mean while thoſe indigenous plants, from which likewiſe a uſeful ſubſtitute for the Weſt India ſugar may be extracted; and it is with this view I have made the following experiments.

1. *Experiments to obtain Sugar from India Corn.*

India corn, (zea mays) is ſaid to contain, according to Vonſuſti,‡ ſugar, particularly in the nodes of the young ſtalks, from which Mr. Jacquin of Vienna,§ has ſucceſsfully prepared it; and this is farther confirmed by Mr. Marabelli ‖ in a diſſertation on the ſubject. It is likewiſe reported, that the extraction of ſugar from the ſtalks of India corn, growing particularly in marſhy ſoils, has been tried in Italy upon a large ſcale, but afterwards left off again, as it was found not to anſwer the purpoſe, the ſugar thus obtained being more expenſive than common raw ſugar. To be convinced by my own experience on this ſubject, I made ſome experiments, of which the following are the reſults: A quantity of India corn was cultivated, in a tolerable and ſomewhat marſhy ſoil, for the purpoſe. When the young plants were about ſix inches high, the

‡ Okonomiſche Schriften, i. e. Economical Writings, Tom i. p. 307, and Tom ii. p. 191.

§ Crell's Chemical Annals, 1784, Vol. I.

‖ Franc. Marabelli de Zea Mays planta, analytica diſquiſitio. Pavia, 1793.

the leaves, when chewed, had a ſweetiſh taſte, but the ſtalks, particularly about the nodes, taſted like ſugar. Theſe young plants being cut off as near the ground as poſſible, freed from the leaves, and ſufficiently cleaned, ten pounds of them were cut in pieces, and being pounded in a ſtone mortar, the juice was expreſſed, which weighed three pounds. This juice, whoſe ſweetiſh taſte had ſtill a diſagreeable flavour of herbs, was clarified with the white of eggs, after which that taſte was ſcarcely perceptible, and being thickened to the conſiſtence of a ſyrup, eight ounces of a very agreeable taſting ſyrup were obtained.

2. *Examination of the Spikes of India Corn.*

As the young ſpikes, when they are beginning to form, poſſeſs a very agreeable ſaccharine taſte, they were thought fit for being examined. Ten pounds of them were accordingly ſqueezed in a ſtone mortar, and the juice expreſſed, after the leaves had been ſtripped off. Theſe gave four pounds of a milky juice, which could not be rendered perfectly clear by the white of eggs. By a ſlow evaporation, to the conſiſtence of a ſyrup, nine ounces of an agreeable taſting brown ſyrup were got, but which differed from the former by being more mucilaginous.

3. *Examination of Stalks of India Corn of a more advanced Growth.*

Twenty pounds of theſe ſtalks were cut in pieces, and, with the addition of water, ſqueezed in a ſtone mortar and the juice expreſſed, which poſſeſſed a diſagreeable and ſomewhat acrid taſte. Being in the ſame manner clarified, and thickened to the conſiſtence of a ſyrup, twelve ounces of ſyrup were obtained, which had a diſagreeable ſaline taſte, and might rather be conſidered as a vegetable extract than as ſugar.

4. *Experiments for obtaining dry Sugar from India Corn.*

To learn whether it was poſſible to exhibit a cryſtallizable ſugar from this plant, the ſyrups prepared from the young ſtalks and the ſpikes were each diſſolved by itſelf in freſh lime-water, and gently boiled, by which a great part of their impurities was carried off. The liquors being ſtrained through a woollen cloth, each of them was boiled to the thickneſs of a ſyrup, which was put in a glaſs, and ſet for eight months in a warm place, when little cryſtallizations of ſugar appeared, which were with difficulty ſeparated from the fluid. For this purpoſe, each ſyrup was evaporated by a gentle fire till it became dry, and this maſs was digeſted with alkoholized ſpiritus vini to ebullition.

ebullition. The fluid, still hot, was instantly poured through some linen, whereon the mucilaginous parts remained; but on cooling of the spiritous solution, a true sugar, of a yellow colour, crystallized in small grains. The alkohol being drawn from the remaining fluid by distillation, another portion of sugar was got by gentle evaporation, and altogether two ounces from the syrup of the young stalks, and one ounce and a half from that of the spikes.

By these experiments it is sufficiently shown, that from the young fresh stalks, as well as from the spikes of India corn, a true sugar can be extracted; but as its separation from the gummy and other particles mixed with it, is attended with such difficulties, and the gain so very inconsiderable, that a pound of raw sugar from this plant would cost one rix-dollar, or above three shillings, it appears that no profit or economy will arise from the fabrication of this sugar.

5. *Experiments for obtaining Sugar from the Siberia Cow-parsnep.*

The Russian cow-parsnep, (*Heracleum sphondylium Linn. Heracleum Sibericum*) has been long known as a plant containing a great deal of saccharine matter, in which respect, according to Steller, (in his Travels to Kamtschatka, in German) it deserves the next place to the sugar-cane, and the natives call it therefore *sweet-herb*, or *katsh*. According to Gmelin (Flora Siberica, tom 1, p. 214.) it does not differ from our common cow-parsnep; but others think it a particular species, to which they give the name of *sphondylium panaces*. The inhabitants of Kamtschatka gather the stalks of this plant in June, and having stripped off the leaves, they shave off the outer skin with muscle shells, and dry them in the sun, and afterwards they are chewed for the sake of sucking out the saccharine matter. In drying, the surface of the stalks is covered with a white saccharine powder, which they separate by shaking them in a leather bag; but 40 ℔. of them afford only a quarter of a pound of this powder sugar, which therefore is considered as a great rarity. Besides this, the stalks and roots of the plants are employed for obtaining a sort of brandy.

I was supplied with some fresh plants of the Heracleum Sibericum for my experiments; but finding that the stalks were by no means so rich in sugar, as it is related of those plants growing in Siberia, I tried the roots, of which I got four pounds, whose taste is sweetish like that of parsneps. Having freed them from the outer skin, they were dried, but no saccharine crust appeared on the surface; they were therefore ground, and being mixed with water, the juice was expressed, which tasted

tasted sweetish but a little acrid. Being boiled with the white of eggs, and clarified, it was thickened to the consistence of a syrup, of which six ounces were got, wherein, after a space of three months, a brown, grainy sugar had crystallized, which, however, was not quite free from a disagreeable flavour.—Though it is shown by these experiments, that sugar may be obtained from that plant, yet the preparation of the sugar is too expensive for making use of it. It is, however, probable that the soil has a great influence upon the plant, and that therefore those growing in Siberia are richer in sugar.

6. *Experiments to obtain Sugar from the Must of Wine.*

It might be presumed from the taste of must obtained from ripe grapes, that a considerable quantity of saccharine matter is contained in it, though involved by mucilage. To try whether a true sugar could be extracted from it, some experiments were undertaken. Eight Berlin quarts of must from ripe sweet grapes, were seethed with the white of eggs, clarified, and filtered. The fluid being evaporated, gave three pounds of an agreeable but acidulous syrup. To take away this free acid, the syrup was dissolved again in lime water, and so much of it added, till no acid was perceived by reagency. The fluid being again clarified and evaporated, a very agreeable syrup was obtained, but from which it was by no means possible to procure crystallized sugar. However, this syrup would not be very profitable at the high price of must.

7. *Experiments with the Juice of the white and black Birch for the sake of obtaining Sugar.*

It has been much doubted, whether sugar might be got from the juice of those trees. Mr. Stæhlhammer, (Transactions of the Swedish Academy, Vol. xxxv.) prepared from the juice of the white birch, *Betula alba*, tapped in the spring, a considerable quantity of syrup, superior to the common brown syrup, but not so good as that from the maple. Mr. Kalm (*Ibid.* Vol. xiii.) relates, that from the North American black birch (*Betuli nigra, carpinifolia*), also called sugar birch, a great deal of sugar is obtained, which is not so sweet as that from the maple. Having received a sufficient quantity of the juice of both trees, I found that from one hundred and fifty Berlin quarts of the juice, extracted in April from fifty trees of the white birch, two pounds and a half of brown syrup were obtained, out of which no crystallized sugar could be got. But fifty quarts of the juice of the black birch gave one pound and a half of a very good and useful syrup, yet inferior to that of the maple. From this syrup a considerable portion of crystallized sugar separated;

parated; by which it appears, that ſugar may be obtained from the black birch, but much inferior in quality to that of the maple, and, at the ſame time, more expenſive.

8. *Experiments to obtain Sugar from the Beet. (Beta eicla alba.)*

It is more than fifty years ſince the late Mr. Marggraff made ſeveral experiments with different ſpecies of the beet, to try how far it was poſſible to obtain a uſeful ſugar and ſyrup from them; in which, however, he ſeems to have particularly ſucceeded with *Beta eicla*, as he extracted from 2 lb. of its freſh roots, half an ounce of good ſugar. Induced by theſe experiments, I took 68 lb. of the roots of beet, which being ſufficiently cleaned, were ground and ſqueezed, and the reſiduum being moiſtened with warm water and again expreſſed, about 20 quarts of juice were obtained. A third part of this having evaporated, it was then mixed with 20 quarts of lime-water, and boiled for half an hour. The juice thus become clear, and of a yellowiſh colour, was filtered, and afterwards thickened to the conſiſtence of a ſyrup, of which 6 lb. were obtained in this way, of a brown yellowiſh colour, and an agreeable taſte. A part of it was poured into an evaporating glaſs diſh, in which ſome glaſs tubes had been placed; and being expoſed for ſixteen weeks in a warm ſituation, thoſe tubes were found to be incruſted with cryſtals of ſugar, about the ſize of a pea, and very much like brown ſugar-candy. A buſhel of the beet roots coſts about half a crown, and the expence of obtaining a ſyrup from them is not much above ſix-pence; a pound of the beet ſyrup would, therefore, come no higher than about four-pence, which is leſs than the price of the common ſyrup in Germany; conſequently, it might prove a very advantageous ſubſtitute. The ſugar may likewiſe be obtained cheap enough, in compariſon to the preſent high price of the cane-ſugar; but the operation to extract it is very long and ſlow; and though it equals the ſugar of the maple in goodneſs, yet the proceſs of extracting it is far more expenſive.

9. *Experiments to obtain Sugar from another Species of Beet, (Beta vulgaris altiſſima, or Beta eicla altiſſima Jacquin.— Bunkelrube, Germ.)*

This ſpecies of beet-root is commonly cultivated in ſome parts of Germany, as a very good food for cattle, and differs from the former by being more ſucculent and of a ſweeter taſte. The roots grow larger, and contain more juice, when cultivated in a rich ſoil. A buſhel of theſe roots, which weighed about 125lb. after the outer rind had been taken off, were ground

ground upon a grater; and after a part of the juice had difcharged itfelf during the grinding, the reft was expreffed; by which means, 24 quarts of a very fweet violet-coloured juice were obtained. Taught by repeated experiments, I firft boiled it to the evaporation of one-third, by which a great deal of albuminous matter feparated. When cool, it was poured through a woollen cloth, mixed with 24 quarts of lime-water, and boiled for half an hour. The juice became in this way a clear fluid of a yellowifh colour, being freed from a great part of its impurities. After having cooled and ftrained it, it was boiled to the thicknefs of a fyrup, of which eight pounds were got. A part of this was likewife expofed in a deep evaporating difh, in which glafs tubes were put, to a gentle evaporation, and after two months a deep yellow fugar was found to have cryftallized on thofe tubes. As the cryftallization of fugar proceeds very flowly in this fyrup, as well as in the former, the quantity of fugar which it may contain cannot be determined; but as this fyrup is by far more agreeable than common fyrup, and moreover as, including all expences, a pound will coft no more than about three halfpence, it may likewife ferve as a ufeful fubftitute. This fpecies of beet is the fame which Mr. Achard, of Berlin, has fince ufed for the fabrication of fugar upon a larger fcale.

10. *Experiments for obtaining Sugar from the red Beet, (Beta-rubra, Beta eicla rubra, Jacquin.)*

The poffibility of obtaining fugar from the common red beet has been already fhown by Marggraff; I proceeded therefore in the fame way as with the former, and obtained from a bufhel of its roots fix pounds and a half of fyrup, which had rather an unpleafant rough tafte, from which, however, it may perhaps be freed. I did not try to get fugar from it, though Marggraff obtained from 8 ounces of dried roots 2½ drachms of fugar.

[To be continued.]

On

On the Introduction of the Vaccine Inoculation at Paris.

Paris, le 14 Germinal (5 Avril, 1800.)

A Monſieur PEARSON, Médecin de l'Hopital de l'Inoculation Vaccine, à Londres.

LES Journaux nous ont fait connoître, Monſieur, les Eſſais faits en Angleterre, relativement à l'Inoculation de la Vaccine et dont les ſuccès ſont dus, *en grande partie, à votre* zèle eclairé. Quelques amis de l'humanité et de la ſcience, que vous cultivez avec tant de diſtinction, ont formè le projet d'introduire en France, une pratique qui paroit promettre de ſi grands avantages. Ils ont propoſé, a cet effet, une ſouſcription qui eſt ſur le point d'être remplie. *Vous en trouverez ci joint le proſpectus.* Mais pour mettre à éxécution les vuês qui y ſont expoſées, nous avons beſoin de votre intervention, et c'eſt avec confiance que nous la ſollicitons.

Quoique l'on ait commencé ici quelques eſſais ſur la Vaccine, dont l'ecole de médecine s'eſt occupèe, plus particulièrement, on n'a pû cependant parvenir encore a aucun réſultat poſitif, la difficulté de ſe procurer de la matière *aſſez récente*, en a ſans doute été la principale cauſe. Nos artiſtes ſemblent ne point connoître cette affection ſur les vaches. Dans une ſeule circonſtance, on a cru reconnaitre la maladie ſur des animaux de cette eſpéce, et on a ſaiſi cette occaſion de recueillir du virus, qui a été employé concurrement *avec de la matière reçue de Geneve et d'Angleterre, mais ſans aucun ſucces encore bien reconnu.* Nous nous trouvons donc prives du premier moyen néceſſaire pour commencer nos eſſais, et *c'eſt a vous que nous nous addreſſons pour nous le procurer.*

Nous deſirons, ſi toutes fois vous jugez ces précautions utiles, recevoir des croutes ou puſtules recueillies ſur les vaches, et ſur les ſujets inoculès, et des fils ou de petites eponges imprégnés de la matière, priſe ſur les uns et ſur les autres, de ces mêmes croutes ou puſtules fraiches et de l'ecoulement qui s'etablit à la plaie formée par l'affection locale de la partie inoculée: les differentes eſpeces de matières ou ſubſtances devroient être diſtinguées par une indication particulière. Si votre methode de conſerver les fils dans le gaz azote ou le gaz hydrogéne, vous paroit de quelqu' avantage, nous attendons *de votre zéle,* que vous voudrez bien l'employer. Nous prenons, avec notre miniſtre des relations extérieures, les meſures convenables, *pour que les moyens d'envoi les plus ſurs, les plus commodes, et ſur tout les plus prompts ſoient à votre diſpoſition.*

sition. La société ne mettra pas moins d'empressement, Monsieur, a faire connoître les services que aurez bien voulu lui rendre et à vous informer du résultat de ses recherches, *dont le mérite vous sera du en grande partie*, si à l'envoi de la matière que nous vous demandons, vous avez la bonté d'ajouter *quelques instructions particulieres*, dont nous nous feliciterons de pouvoir profiter.

Salut, *Estime* et *Devouement*, les Membres composant le Comité Médical de la Société formée à Paris, pour l'Inoculation de la Vaccine.

Thouret, Directeur de l'Ecole de Medecine.
Pinel, Prof. de l'Ecole de Medecine.
Rouselle Chamseru, Med. de l'Armée.
Parfait, Chir. Inoc.
Tessier, de l'Institut.
De la Port, Med. des Hopit. Milit.
Huzard, de l'Institut.
Cabannez, de l'Institut. et Prof. de l'Ecole de Med.

The above letter, as appears, was addressed to Dr. Pearson, who, however, in place of returning an answer in his own name only, presented it to the *Vaccine Institution*, where it was determined that the commission should be executed by the *Medical Establishment.* Accordingly a packet, containing vaccine matter on *glass*, on *lancets*, and on *thread*, inclosed in a bottle filled with hydrogen gas, and a letter of instructions, signed by all the members of the Medical establishment, were sent to Paris, on the 12th of May last, with the permission of Lord Grenville, through the French Commissioner, Mr. Otto.

How well the vaccine matter, thus forwarded, answered the sanguine expectations of the Parisian Physicians, notwithstanding the journey and delay, is sufficiently proved by the following Extract from the Gazette Nationale; and other French papers furnish similar reports.

Gazette Nationale, 23 Prarial, An. 8, de la Republique Françoise, une et indivisible.

Sur l'Inoculation de la Vaccine.

Le 13 Prarial l'Inoculation de la Vaccine à eu lieu sur *trente Enfans*, avec la matière envoyée d'Angleterre et d'apres les procédes recommandés. Des signes d'infection se sont manifestés *sur neuf d'entre eux* aux époques et avec les caracteres annonces par le Docteur Pearson, et d'autres membres du Comité institué à Londres pour l'Inoculation de la Vaccine. Il est a remarquer que les Medecins Anglois avoient exprimé, dans leur lettre, "Que l'on devoit se croire heureux, vu le

"laps

" laps de tems entre la collection de la matière et de son ap-" plication, si, sur vingt individus inoculés, un seul prenoit la " maladie." Le 19, le 20, et le 21, *dix-huit enfans* sur lesquels la premiere Inoculation n'avoit point eu d'effet, ont été inoculés de nouveau avec la matière recueillie sur ceux chez qui la premiere inoculation avoit pris. Le Comité continue ses essais et instruira le public des resultats ulterieurs.

Pour le Comité Médical,
THOURET.

VACCINE INOCULATION, from the Official Report at PARIS.

Inoculation House at Vaugirard, July 27, 1800.

We have now got Dr. WOODVILLE, the Physician to the Inoculation Hospital at London. This learned man, to whom we are indebted for an Essay on the Cow-pox, translated into French by Dr. Albert, animated by a generous zeal, and deserving in every respect our gratitude and praise, is come to assist our experience in Vaccine Inoculation.

His practice has constant and invariable success, as more than 6000 children inoculated by the cow-pox matter, and preserved from the Small-pox, promise us effects equally satisfactory.

Does not this preservative from the usual disease seem, by its beneficent quality, to be a kind of prodigy, when we consider that the labour it gives rise to is nothing more than the puncture which one makes for the purpose of inoculating, and is exempt from the slightest accident.

COLON, M. D. Member of the Vaccine Medical Committee.

The Vaccine Institution, which has already been so useful in England, and communicated with various parts of Europe, to introduce the Cow-pock Inoculation, has sent the following notice.

Vaccine Inoculation Institution, Warwick Street, Golden Square. July 28, 1800.

From the increase of patients, and the numerous applications for Vaccine matter, the Subscribers are hereby informed, that it has been resolved to inoculate every Friday as well as Tuesday, under the direction of the Medical Establishment, which at present is as follows:

G. PEARSON, M. D. F. R. S. L. NIHELL, M. D. and T. NELSON, M. D. Physicians.

THO. KEATE, Esq. F. R. S. and JOHN RUSH, Esq. Consulting Surgeons.

R. KEATE, Esq. J. GUNNING, Esq. and J. C. CARPUE, Esq. Surgeons.

A. BRANDE, Esq. F. RIVERS, Esq. and Mr. E. BRANDE, Visiting Apothecaries.

Mr. J. LEWIS, Resident Apothecary, to whom letters (post paid) may be addressed.

The Public are desired to notice, that the Medical Establishment do not authenticate Vaccine matter unless it be delivered under the appropriate seal of the Institution. By order,

W. SANCHO, Secretary.

CRITICAL RETROSPECT
OF
MEDICAL AND PHYSICAL LITERATURE,
[FOREIGN AND DOMESTIC.]

Observations on the Cow-Pox, by WILLIAM WOODVILLE, M. D. Physician to the Small-Pox and Inoculation Hospitals. 8vo. pp. 43. London, W. Phillips.

WHEN the Vaccine Inoculation began to engage the attention of the public, it was immediately perceived by its nature, that two very important pillars of its temple rested on the absence of eruptions, and the impossibility of the contagion being communicated, except by insertion of the virus, or inoculation. The inconvenience arising from these sources had always been considered as such formidable objections to the Small-pox, that very expensive establishments had at that time been begun on the continent, with a view to exterminate it altogether.* When Dr. Jenner therefore, found one part of his immortal discovery to appear defective in this respect in the vicinity of the metropolis, and in those patients particularly who were inoculated with matter sent from thence, and scarcely any others, it was impossible for him, in his second publication on the subject, to avoid offering some conjecture on the cause of this diversity. In doing this, he *criminated* no man; who indeed could criminate such a character as Dr. Woodville? We are therefore much hurt to observe, that Dr. W. should find himself "unable to avoid a certain degree of *recrimination*, which attaches to a man, for whom he has long entertained a friendly regard, and to whom the public is under the great obligation of having been made acquainted with a discovery, which promises the most important benefits to society." There is a gentle mildness of temper, in which the cause of truth delights; but which we do not find so conspicuous in the first part of this pamphlet as we expected. With respect to the refutation of Dr. Jenner's conjecture, the public will judge for themselves when they have read Dr. W's letter.

The following passage, we apprehend, admits every thing that Dr. Jenner supposed:—"Although I differ in opinion from Dr. Jenner in not imputing the pustular eruptions, produced in the cases at the hospital, to any adulteration of the vaccine matter employed in the inoculations, yet I readily admit that they have been and still continue to be the effect of some adventitious cause, independent of the Cow-pox.

"This

* See Number XVII. p. 99.

"This will clearly appear from the following obſervations, which likewiſe tend to place the ſubject in a new light.

"I had not long practiſed the vaccine inoculation at the hoſpital, before I was requeſted to extend it into private families in the metropolis, where I ſoon diſcovered that the Cow-pox uniformly appeared in its mildeſt form, and was never attended with eruptions. I alſo ſupplied ſeveral medical gentlemen with the vaccine matter, which was uſed by them with the like reſult. Hence I began to ſuſpect that there exiſted ſome *peculiar* cauſe, which rendered the patients under the vaccine inoculation in the hoſpital more liable to puſtules than others: and that this ſuſpicion was well founded I have ſince, from daily experience, been fully convinced.

"At various times I procured the vaccine virus, as produced in different cows, and with it inoculated patients in the hoſpital; but the effects of all the matter I tried were perfectly ſimilar: and puſtules proved to be not leſs frequently the conſequence of theſe trials than of thoſe made with the matter formerly employed.

"The laſt matter of the vaccine poiſon which I introduced into the hoſpital, was obtained from Dr. Jenner, and originally taken from Clark's cow, before noticed: with this matter I inoculated at the hoſpital on the ſame day three patieents, on one of whom about 100 variolous-like puſtules were produced. This inſtance, and numerous others of the like kind which I could adduce, decidedly prove, that where there can be no doubt entertained of the purity of the Cow-pock matter, with which the patients in the hoſpital are inoculated, puſtules will frequently be the conſequence.

"On the other hand I have to obſerve, from daily experience during the laſt twelve months, that among the great numbers of children reſiding in various parts of London, to whom I have transferred the Cow-pock infection, no inſtance of puſtules that maturated has occurred. Now, as theſe different effects of the diſeaſe between the patients in, and thoſe out of the hoſpital did not depend upon any difference or alteration of the matter with which the inoculations were performed, the only cauſe remaining to which the frequent occurrence of puſtules on the former can be rationally referred, is the variolated atmoſphere of the hoſpital, which thoſe patients were neceſſarily obliged to inſpire during the progreſs of the Cow-pox infection.

"Did it not lead me too much into detail, I ſhould ſhow from many circumſtances relating to the patients in the Inoculation Hoſpital, that other reaſons might from thence be adduced to ſupport the opinion here advanced.

"Mr. Evans, Surgeon, at Ketley, in Shropſhire, is the only perſon, except myſelf, who has given an account of the variolous and vaccine inoculations carried on ſeparately in different perſons at the ſame time, and in the ſame houſe, ſo that ſeveral of his patients, while under the vaccine infection, were expoſed to the variolous effluvia. The number of thoſe which he inoculated for the Cow-pox

Cow-pox amounted to ſixty-eight; and it is worthy of remark, that more than one-half of theſe patients had puſtules. It is true that the eruptions very rarely maturated; but ſtill their frequent occurrence would ſeem to ſhow they aroſe from the ſame cauſe as thoſe at the hoſpital. I ſuſpect alſo, that in thoſe places where the Small-pox is epidemic, or very generally prevailing, the Cow-pox will be found to be equally liable to excite puſtules as in the hoſpital.

"During the very general and fatal prevalence of the Small-pox at a village eight miles diſtant from London, more than 100 perſons were inoculated under my direction for the Cow-pox, of whom one in five had eruptions; and as theſe furniſh the only inſtances which I have experienced, out of the hoſpital, of the Cow-pox producing the variolous-like puſtules, I am diſpoſed to attribute them to the adventitious co-operation of the variolous atmoſphere to which the patients were expoſed. In what way the variolous miaſms act in thus modifying the Cow-pox, or why they co-operate in ſome and not in all caſes of vaccine infection, I ſhall not even venture a conjecture: the cauſes probably will continue as inexplicable as thoſe conſtitutional peculiarities which produce all the varieties of Small-pox."

The practical importance of the following facts, will be a ſufficient apology for our inſerting them.

"In order to ſhow that thoſe who had undergone the Cow-pox reſiſted the infection of the Small-pox, I obſerved in my *Reports*, that upwards of 400 of the patients who had received the former diſeaſe, were afterwards inoculated for the latter, which in no inſtance was produced; though more than 100 of the patients had the vaccine diſeaſe ſo very ſlightly, that it neither produced any perceptible indiſpoſition nor puſtules. In addition to this, I can now ſay, that more than 1000 of thoſe who had undergone the new inoculation, have been put to the ſame teſt, and that the like reſult has been experienced.

"The above facts, added to a multiplicity of others of a ſimilar import, publiſhed by ſeveral profeſſional men, clearly demonſtrate, that the Cow-pox inoculation promiſes moſt important benefits to ſociety; and under this conviction I congratulate the public on the great progreſs it is making, by which the real value of the invention will ſoon become generally acknowledged, and duly appreciated.

"Thoſe who have had much experience in inoculating with the matter of the vaccine pock, muſt have obſerved that it is more apt to fail in communicating the infection than variolous matter, eſpecially if it be ſuffered to dry upon the lancet before it is uſed. This does not ſeem to depend upon the virus of the former being more volatile and more eaſily carried off by evaporation than that of the latter, but from its becoming more hard and leſs diſſoluble upon exſiccation. Care ſhould therefore be taken to moiſten it a conſiderable time before it is uſed. When fluid matter is employed, the lancet ſhould be held nearly at a right angle with the ſkin, in order that the infectious fluid may gravitate to the point of the

the instrument, which in this direction should be made to scratch the cuticle repeatedly, until it reach the true skin, and become tinged with blood. This method has many advantages over the common puncture, and I have found it a more convenient and effectual mode of performing the inoculation than any other. It may be remarked, however, that there are persons who have never had the Small-pox, and are incapable of receiving it by inoculation, or by any other means whatever. The proportion of these to those liable to the disease has been differently stated by authors; I have not found them to be more than about one in sixty; but as such persons also resist the infection of the Cow-pox, the inoculation of the latter must therefore sometimes fail, independently of the mode in which it is performed, or of the matter employed."

Reflections on the Cow-pox, illustrated by Cases to prove it an absolute Security against the Small-pox; addressed to the Public, in a Letter to Dr. Jenner, from William Fermor, Esq. 8vo. pp. 47. London. Robson and Robinson.

THE first part of this Letter contains general observations on the Small-pox, and the preference due to the Cow-pox, the marks by which the genuine Cow-pox is distinguished, and a statement of the opinions of others respecting its power of permanently preventing the variolous infection.

The author then observes, "That the genuine Cow-pox is a certain preservative against the Small-pox, I flatter myself, the following cases will sufficiently prove. They are selected from many I could bring, of persons who having previously had the Cow-pox, have never been able to receive the infection of the virus from the Small-pox, though inserted a considerable time after, and at different periods. These cases are well known to practitioners and inoculators in this neighbourhood, and I have received most of them from the parties themselves.

"First Case. Jeoffry Tredwell is a reputable farmer, and a tenant of mine, at Chesterton, in this neighbourhood. His brother, William Tredwell, being employed constantly in milking the cows, was infected with the Cow-pox, and had the disease severely in his hands and fingers. Jeoffry not being engaged so much in milking as his brother, did not receive the infection. About three years after, these two brothers were inoculated with variolous matter, by Mr. Lister, of Charlbury, an eminent practitioner, at a house appropriated for that purpose. William Tredwell, who had undergone the Cow-pox, could not receive the infection, though he was inoculated several times, and remained in the house with the other patients. Jeoffry, who had not been infected with it, had a very full Small-pox eruption.

"Second Case. Alban Collingridge had the Cow-pox about five or six and twenty years ago, at his father's farm, at Poodle, which affected his fingers in a violent degree. About four years after, he was three times inoculated for the Small-pox, by Mr. Lister, without

out effect. Two of his brothers, who had never had the Cow-pox, received the variolous infection. He slept with them in order to take it, but no consequence ensued. He has frequently since been exposed to its contagion, and has very lately inoculated his children with the Small-pox, without being in any shape infected with it himself.

"Third Case. Mr. Stevens, a reputable farmer of East Claydon, in the county of Bucks, had the Cow-pox on his farm, in the year 1764.—He himself was infected with it by milking the cows. About four years after, he was inoculated with variolous matter, but without effect. About the year 1791, his family were inoculated with the rest of the parish for the Small-pox, with which they were all infected, but he was not, though he attended them the whole time. This case must appear decisive with regard to the security the Cow-pox matter affords against the variolous infection, as there was a space of twenty-seven years between his having received the disorder from his cows, and his attending his family in the inoculated Small-pox; and an interval of four years between the time he had the Cow-pox, and his being himself inoculated with the Small-pox without effect."

Many other cases are given, equally conclusive with the above; and a list of 326 persons who were inoculated for the Cow-pox, of whom 173 were afterwards inoculated with Small-pox, without receiving the infection in any instance.

Mr. F. concludes his Letter with the following recapitulation: "It is unnecessary for me to say any more on this truly interesting subject. I leave the impartial reader to his natural reflections; but I think, in consequence of the above premises, I may venture to say, that he will now be of opinion that the genuine Cow-pox is mild in its effects, congenial with every situation and employment of life, totally void of contagion, and a certain preservative against the baneful influence of the Small-pox. That no constitutional or family complaint can interfere with its effects, or prevent its adoption.

"To conclude; though the public have certainly great obligations to many distinguished modern practitioners, for having, by the cool regimen and present mode of treatment, considerably abated the natural virulence of the Small-pox, to you alone we are certainly indebted for its complete annihilation."

Medical Jurisprudence. On Madness; by JOHN JOHNSTONE, M.D. 8vo. pp. 56. London, 1800. Johnson.

THE Author informs us, that this is a part of an extensive work on the subject of medical jurisprudence, and which he should not have published separately, had he not been induced by some recent events. "I have not aimed," says he, "at collecting much of what has been said on the subject of insanity by others. The Anatomy of Melancholy, the works of Battie, Arnold, and Crichton, have exhausted all that can be quoted on the subject, and with Haslam's Practical Inquiry, and the Philosophy of Zoonomia, nearly

ly the whole that has ever been ſaid. To theſe writings therefore the inquiſitive reader may refer. I have made an effort indeed to compreſs, not to expand my materials; and ſuch as they are, I now deliver them to the public, with the hope that they will tend to familiariſe the ſcientific doctrines of inſanity."

The Author thus ſums up his opinions and advice on the ſubject of Inſanity. "Thus then it appears, that melancholy, lunacy, inſanity, madneſs, are the ſame diſeaſe—a diſeaſe of the organs of the mind, often called into action by vehement paſſions, or by injuries of different organs of the body.

"Madneſs can only be deemed an hereditary diſeaſe, inſomuch, as children have a ſtructure ſimilar to that of their parents who have been mad, and as this peculiar organization is likely to be acted upon by the peculiar manners and habits of the parent in education.

"Madneſs has no lucid intervals; a man is either inſane or not inſane at a particular moment: unleſs indeed we be allowed to term every period, in which the hallucination of the maniac does not appear, a lucid interval. But this would be moſt abſurd, for madneſs is a diſeaſe of the brain, and ſenſorial powers, and ſeldom diſcovers itſelf equally at all times. Madneſs is not always diſtinguiſhable from manner—for it aſſumes the form of the character, whatſoever that may be.

"The countenance of maniacs is marked by a peculiar wild ſtare, not to be miſtaken by experienced perſons, generally mixed with a ſuſpicious or timid, and ſometimes with a furious look. Their health is not always viſibly affected, though, for the moſt part, the fibres of maniacs, or their powers of motion, are leſs irritable or mobile than in good health; hence they are coſtive, and difficult to be purged or vomited. Their ſenſorial powers, in ſome meaſure, benumbed; hence they feel pain with leſs acuteneſs, and are capable of bearing great extremes of heat and cold, hunger and thirſt. Their pulſe is generally ſlower than common, when there is no irritation nor diſorganization.

When it is determined that a man is mad, he ought to be ſuppoſed incapable of acting. He may perchance act wiſely, but reaſon being abſent, it is ſolely from accident if he does ſo. A maniac cannot commit crimes, and therefore he ought not to be amenable to human law for their commiſſion. He does not diſcriminate right from wrong.

"All maniacs ſhould be controuled, but all do not require confinement. The neceſſity of confinement muſt be determined by the degree of fury, by the temper, and the habits of the maniac.

"Finally, Maniacs ſhould never be entruſted with the management either of themſelves, or any other perſons, eſpecially the young. There is no faculty more familiar to us than that of imitation; it is the firſt exerciſed by the infant, and it grows with his growth. Maniacs therefore ſhould not be ſuffered to aſſociate with young perſons, who will be likely to imitate their actions. For by reiterated imitation, by ſlow yet certain ſteps, we acquire habits,

which not only fix the moral character of man, but frequently produce the most pernicious and incorrigible diseases, both of body and mind."

The Edinburgh Practice of Physic and Surgery; preceded by an Abstract of the Theory of Medicine, and the Nosology of Dr. Cullen; and including upwards of Five Hundred authentic Formulæ, from the Books of St. Bartholomew's, St. George's, St. Thomas's, Guy's, and other Hospitals in London, and from the Lectures and Writings of the most eminent public Teachers. With four Quarto Plates, neatly engraved, representing the different Instruments used in Surgery. 8vo. pp. 920. price 14s. in boards. London, 1800. Kearsley.

THIS comprehensive Compilation may be considered as a Medical Library for young students; and we think it may, for some time, supercede the necessity of any other. It will serve the purpose of a Text-book during their attendance on Medical and Surgical Lectures, and we may add Midwifery; for the diseases of pregnancy, the puerperal state, and those peculiar to infants, are contained in it. We also find several diseases not treated of by systematic writers in general, but only in distinct works, as Angina Pectoris, Poisons, Suspended Animation, Cow-pox, &c. The work is furnished with Indices to the Practice of Physic, the Surgery, the Materia Medica, and the doses of medicines, as well as of the new and old names.

We recommend this book as a valuable addition to the library of the apothecary's shop.

Nosology: or, A Systematic Arrangement of Diseases, by Classes, Orders, Genera, and Species; with the distinguishing Characters of each, and Outlines of the Systems of Sauvages, Linnæus, Vogel, Sagar, and Macbride. Translated from the Latin of W. Cullen, M. D. late Professor of the Practice of Physic in the University of Edinburgh. 8vo. pp. 238. London, 1800. Robinsons.

THIS Translation appears to us to be executed with great accuracy and fidelity; and is accompanied with the necessary Indices, and a Table of the Greek derivations of the names of the classes, orders, and genera.

A Practical Inquiry into Disordered Respiration; distinguishing the Species of Convulsive Asthma, their Causes, and Indications of Cure. The second Edition, corrected, with an Appendix. By ROBERT BREE, M. D. 8vo. pp. 300. London, 1800. Robinsons.

THE design of this industrious Author is to ascertain the real causes of Asthma, and thence the species and method of treatment. The first edition, he informs us, "was published with some precipitation, and had the faults of diffuseness, and want of perspicuity; but its defects did not prevent the sanction of it in this country, nor on the continent, where it has been translated into two languages."

After a comprehensive view of the opinions of others respecting the *ratio symptomatum*, and stating his objections to the doctrine of spasmodic

ſpaſmodic conſtriction, Dr. B. at p. 88, proceeds to the *diagnoſis*, which we ſhall tranſcribe as a ſpecimen of the reaſoning and ſtile.

" Aſthma is ſo ſtrongly marked, that there can be little difficulty in knowing the diſeaſe; yet it will elucidate the ſubject, to ſhow its diſtinction from ſome other diſeaſes to which it may bear an analogy in its cauſes and effects.

" Defluxions on the upper part of the lungs and Sneiderian membrane are uſually inflammatory affections of the mucous glands which line the paſſages of the noſe, fauces, and trachea, as far as its diviſions, and poſſibly lower in the breaſt, but not extending to the extremities of the air pipes.

" The catarrhal diſpoſition is very frequently followed by Aſthma, becauſe repeated inflammation of the capillary veſſels and mucous excretories, may induce, in ſome habits, a loſs of tone, which may prevent their reſiſtance to a circulatory impulſe even leſs than healthy, and ſubject them to the influence of exciting cauſes of little force in compariſon with what they formerly ſubmitted to. For this reaſon, elderly perſons have their natural excretion of mucus much more copious, as they may have been more affected by catarrh, and they are accordingly more liable to Aſthma. If inflammatory diſpoſition be not wholly loſt in theſe perſons, by the progreſſive debility of the veſſels of the lungs, Peripneumonia Notha will be the character of the pulmonary diſeaſe, attended often with great danger.

" I am aware that this ſpecies will be deſignedly confounded by many reaſoners with the Humoral Aſthma; but it is time that the diſtinction of *Humoral* and *Convulſive* ſhould be better underſtood. If mucus be diſcharged in greater quantity in one caſe than in another, the reſpiratory actions being the ſame in both, there is no good reaſon for calling one only Convulſive. Is not every Humoral Aſthma Convulſive? If the unfortunate patient have ſo little irritability, as not to be excited to cough and expectorate, the phlegm muſt ſuffocate him, if the abſorbing veſſels do not carry it off: and this proceſs is attended by Convulſive reſpiration. But though the Humoral muſt be Convulſive, the Convulſive Aſthma is not always Humoral; for we ſhall ſee that irritation may exiſt in a more ſubtle form than lymph.

" It is conſiſtent with the rules of the animal œconomy, that Catarrh ſhould not be indicated by thoſe violent contractions of the muſcles of reſpiration which take place in Convulſive Aſthma. Fever attends both Catarrh and Phthiſis; and we may obſerve, though we cannot aſſign a reaſon for the fact, that if fever ſuperſedes, it generally terminates convulſive motions.

" If Catarrh occaſionally lead to Aſthma, it ſtill oftener brings on *Phthiſis*, a diſeaſe which depends on a ſtate of the lungs, oppoſite to that which permits ſerous effuſion.

" In Aſthma, an exceſs of blood in the pulmonary veſſels may very probably precede the exhalation of the finer part into the veſiculæ and bronchia; this plethora is local, ariſing from the relaxed texture of the coats of the veſſels, and relieves itſelf by effuſion.

" In incipient *Phthiſis* the arterial impulſe is more conſiderable than in health, but the pre-diſpoſition of the pulmonary veſſels is not favourable to a relief by effuſion, till the fever has acquired ſtrength, and coagulable lymph inſtead of pelucid ſerum comes to be effuſed.

" It is therefore to be allowed, that there is a prediſpoſition, in conſequence of which inflammation will affect the arterial extremities, and produce Phthiſis, as doubtleſs there is a pre-diſpoſition leading to that atonic ſtate of the vaſcular pulmonary ſyſtem producing Aſthma.

" There may be alſo an intermediate ſtate, in which a balance is preſerved between the criſis of inflammation ſealing up the orifices of the arterial exhalents, and their diſtenſion ſo gradually acquired as to permit the eſcape

escape of the finer fluid, and consequent relief of arterial fulness; but it is probable that this balance cannot be long adjusted where the pre-disposing causes have had a considerable influence, and that if the exhalents do not dilate soon in consumptive habits, Phthisis must take place; and in persons of an opposite constitution, which I conceive is favourable to Asthma, the effusion of lymph into the vesiculæ and bronchia will determine, in no long time, the future character of the pulmonary disease.

" *Lethargic affections* have been considerably allied to pulmonary complaints in the consideration of many authors; so much so, as to create a question if the cause of lethargy did not exist in the lungs.

Hippocrates says, " lethargic diseases are the same as pneumonic, and not " altogether different from the humid peripneumony." (Peripneumonia Notha.) Some of his commentators have even defended this opinion by the practical remark, that lethargy is critically relieved by expectoration of purulent or serous fluid.

" The lethargic symptoms in Asthma and Peripneumony are sufficiently accounted for by the interruption to the course of the blood from the right side of the heart to the left, obstructing the influx of venous blood from the head. The natural consequence in bad cases of Asthma is Apoplexy.

" Asthma being thus occasioned by serum in the vesiculæ, may be considered as an *Hydropic Disease*; but it is obviously distinct from Hydrothorax, in which the water is collected in the sacs of the pleura, or cellular texture of the lungs. In each situation it will occasion dyspnœa, which, though subject to exacerbations, from accidental causes, will not put on the form of Periodic Asthma.

" We have sufficient testimony of the connection of Asthma with Dropsy in the histories of practical authors, frequently pointing out the intercurrence of symptoms and changes of one disease into the other, when Asthma has been of long standing: some proof of this is contained in Sect. VI. of this Inquiry.

" Hoffman and Willis have particularly noticed the hydropic appearances of the feet, and the tendency to general Dropsy in Asthma: and the observation of these authors is supported by that of other practitioners.

" Sydenham opens his treatise of the Dropsy, by stating the first symptom of that disorder, to be the swelling of the legs, and pitting of the ancle by pressure of the finger: but this is not so certain a sign in women as in men; nor even in the latter is it to be considered as an absolute certainty of the disease having commenced. He then proceeds with the following observation:

" Etenim cum senex quispiam, habitu corporis paulo pleniori præditus, " *Asthmate* jam a multis annis laborans, ab eodem derepente, idque hye- " mis tempore, fuerit liberatus, mox ingens tumor musculos tibiarum oc- " cupabit, hydropicorum tumores æmulans, qui hyeme etiam magis quam " æstate, tempestate magis pluviâ quam serenâ, pariter invalescit, et tamen " sine quovis incommodo insigniori, eundem ad libitinam usque comitabi- " tur. Quo non obstante, si generaliter loquamur, suræ et tibiæ intumes- " centes, etiam in viris, pro signo supervenientis hydropis habendæ sunt; " maxime si ita affecti spiritum ægrius ducant."

" This sagacious observer might have attributed the swelling of the legs, with great truth, to hydropic effusion in the Asthmatic, as well as in other cases; and the cessation of the Asthma when these swellings commenced, seems to corroborate, beyond dispute, the theory, that both affections depended on one cause. The swelling was larger in winter than in summer, in moist weather than in dry. Alterations in the atmosphere rapidly affect the Asthmatic, and change his habit from a perspiring to an imbibing state, and whether the aqueous collection stagnate in the vesicles of the lungs, or

be

be taken up by the absorbents, and be again effused in the lower extremities, the identity of the cause is sufficiently plain.

"Insanity sometimes suspends Consumption, and Consumption Insanity: Asthma, likewise, is succeeded occasionally by Insanity; probably from the turgid state of the vessels of the head in consequence of the difficulty with which the right side of the heart propels the blood through the lungs to the left. In the hydropic diathesis, so frequently accompanying advanced Asthma, the disease of the head is still more frequent.

"The following two cases deserve attention. In one, the patient had alternately Asthma and Insanity: in the other, there appeared Anasarca and Insanity at the same time. The treatment was successful, though founded on the sole indication of curing the Dropsy.

"We conclude then, that Asthma, Insanity, and Dropsy, had the same cause; for, if Insanity and Asthma were one disease, and Insanity and Dropsy were one disease, *Asthma* and *Dropsy* must be one disease.

"From a consideration of the causes or Insanity by the learned Dr. Arnold, there can be no difficulty in assenting to the connection between these diseases."

The Author next proceeds to the investigation of the predisposing and exciting causes, and the establishing of his *four* species. He then lays down a plan of treatment for the paroxysm of each species, with general rules for the *diet* of the patients, both during the paroxysms and the intermissions.

The Appendix contains a recapitulation or synopsis of the work, with additional facts and observations in confirmation of his distinctions, and plan of treatment.

Though the stile of this work appears to us defective, we have no hesitation in recommending the matter of it to practitioners, as well worthy of their attention.

Researches, Chemical and Philosophical; chiefly concerning Nitrous Oxyde, or Dephlogisticated Nitrous Air, and its Respiration. By HUMPHRY DAVY, Superintendent of the Medical Pneumatic Institution. 8vo. pp. 590. London, 1800. Johnson.

THE first of these Researches chiefly relates to the production of nitrous oxyde, and the analysis of nitrous gas and nitrous acid. "In this," says the Author, "there is little that can be properly called mine; and if by repeating the experiments of other chemists, I have sometimes been able to make more minute observations cconcerning phænomena, and to draw different conclusions, it is wholly owing to the use I have made of the instruments of investigation discovered by the illustrious fathers of chemical philosophy, Cavendish, Priestley, Black, Lavoisier, Scheele, Kirwan, Guyton, Berthollet, &c. and so successfully applied by them to the discovery of truth.

"In the second Research, the combinations and composition of nitrous oxyde are investigated, and an account given of its decomposition by most of the combustible bodies.

The third Research contains observations on the action of nitrous oxyde upon animals, and an investigation of the changes effected in it by respiration.

In the fourth Research the history of the respirability and extraordinary

ordinary effects of nitrous oxyde is given, with details of experiments on its powers made by different individuals."

Our recommendation will not be necessary to induce every lover of chemistry and physiology to peruse this truly philosophical work; our readers will judge of its importance by the following extracts.

Mr. D. concludes his first Research with the following general remarks on the production of nitrous oxyde. "There are no reasons for supposing that nitrous oxyde is formed in any of the processes of Nature; and the nice equilibrium of affinity by which it is constituted, forbids us to hope for the power of composing it from its simple principles. We must be content to produce it, either directly or indirectly, from the decomposition of nitric acid. And as in the decomposition of nitrate of ammoniac, not only all the nitrogene of the nitric acid enters into the composition of the nitrous oxyde produced, but likewise that of the ammoniac, this process is by far the cheapest, as well as the most expeditious. A mode of producing ammoniac at little expence, has been proposed by Mr. Watt. Condensed in the sulphuric acid, it can be easily made to combine with nitric acid, from the decomposition of nitre by double affinity. And thus, if the hopes which the experiments at the end of those researches induce us to indulge, do not prove fallacious, a substance which has been heretofore almost exclusively appropriated to the destruction of mankind, may become, in the hands of philosophy, a means of producing health and pleasurable sensation."

Respecting the decomposition of nitrous oxyde and its analysis, he draws these general conclusions. "From what has been said in the preceding sections, it appears that the inflammable bodies, in general, require for their combustion in nitrous oxyde, much higher temperatures than those at which they burn in atmospheric air, or oxygene.

"When intensely heated they decompose it, with the production of much heat and light, and become oxygenated.

"During the combustion of solid or fluid bodies, producing flame, in nitrous oxyde, nitrous acid is generated, most probably from a new arrangement of principles, analogous to those observed in Sect. II. by the ignition of that part of the gas not in contact with the burning substance. Likewise when nitrous oxyde in excess is decomposed by inflammable gas, nitrous acid, and sometimes a gas analogous to common air, is produced, doubtless from the same cause.

"Pyrophorus is the only body that inflames in nitrous oxyde, below the temperature of ignition.

"Phosphorus burns in it with the blue flame, probably forming with its oxygene only phosphoreous acid at the dull red heat, and with the intensely vivid flame, producing phosphoric acid at the white heat.

"Hydrogene, charcoal, sulphur, iron, and the compound inflammable bodies, decompose it only at heats equal to, or above, that of ignition; *probably*, each a different temperature.

"From

"From the phænomena in Sect. V. it appears, that at the temperature of intense ignition, phosphorus has a stronger affinity for the oxygene of nitrous oxyde than hydrogene; and reasoning from the different degrees of combustibility of the inflammable bodies, in mixtures of nitrous oxyde and nitrogene, and from other phænomena, we may conclude with probability, that at about the white heat, the affinity of the combustible bodies for oxygene takes place in the following order: Phosphorus, hydrogene, charcoal, iron, sulphur, &c.

"This order of attraction is very different from that obtaining at the red heat; in which temperature charcoal and iron have a much stronger affinity for oxygene than either phosphorus or hydrogene.

"The smallest quantity of oxygene given in the different analyses of nitrous oxyde just detailed, is thirty-five hundred parts; the greatest proportion is thirty-nine.

"Taking the mean estimations from the most accurate experiments, we may conclude that 100 grains of the known ponderable matter of nitrous oxyde, consist of about 36,7 oxygene, and 63,3 nitrogene; or, taking away decimals, of 37 oxygene to 63 nitrogene; which is identical with the estimation given in Research I.

"Analysis and synthesis clearly prove that oxygene and nitrogene constitute the known ponderable matter of atmospheric air, nitrous oxyde, nitrous gas, and nitric acid.

"That the oxygene and nitrogene of atmospheric air exist in chemical union, appears almost demonstrable from the following evidences.

"1st. The equable diffusion of oxygene and nitrogene through every part of the atmosphere, which can hardly be supposed to depend on any other cause than an affinity between these principles.

"2dly. The difference between the specific gravity of atmospheric air, and a mixture of 27 parts oxygene and 73 nitrogene, as found by calculation; a difference apparently owing to expansion in consequence of combination.

"3dly. The conversion of nitrous oxyde into nitrous acid, and a gas analogous to common air, by ignition.

"4thly. The solubility of atmospheric air un-decompounded in water.

"Atmospheric Air, then, may be considered as the last intimate of the combinations of nitrogene and oxygene.

"It is an elastic fluid, permanent at all known temperatures, consisting of ,73 nitrogene, and ,27 oxygene. It is decomposable at certain temperatures, by most of the bodies possessing affinity for oxygene. It is soluble in about thirty times its bulk of water, and as far as we are acquainted with its affinities, incapable of combining with most of the simple and compound substances. 100 cubic inches of it weigh about 31 grains at 55° temperature, and 30 atmospheric pressure.

"Nitrous Oxyde is a gas unalterable in its constitution, at temperatures below ignition. It is composed of oxygene and nitrogene,

trogene, exifting *perhaps* in the moft intimate union which thofe fubftances are capable of affuming. Its properties approach to thofe of acids. It is decompofable by the combuftible bodies at very high temperatures, is foluble in double its volume of water, and in half its bulk of moft of the inflammable fluids. It is combinable with the alkalies, and capable of forming with them peculiar falts. 100 grains of it are compofed of about 63 nitrogene, and 37 oxygene. 100 cubic inches of it weigh 50 grains, at 55° temperature, and 30 atmofpheric preffure.

"Nitrous Gas is compofed of about 56 oxygene, and 44 nitrogene, in intimate union. It is foluble in twelve times its bulk of water, and is combinable with the acids, and certain metallic folutions; it is poffeffed of no acid properties, and is decompofable by moft of the bodies that attract oxygene ftrongly, at high temperatures. 100 cubic inches of it weigh about 34 grains, at the mean temperature and preffure.

"Nitric Acid is a fubftance permanently aëriform at common temperatures, compofed of about 1 nitrogene, to 2,3 oxygene. It is foluble to a great extent in water, and combinable with the alkalies, and nitrous gas. It is decompofable by moft of the combuftible bodies, at certain temperatures. 100 cubic inches of it weigh, at the mean temperature and preffure, nearly 76 grains."

The third Refearch contains a great number of experiments on the changes produced in them, by the refpiration of nitrous oxyde, and other gaffes; and concludes thus:

"The experiments in the firft Divifion of this Refearch, prove that nitrous oxydes when refpired by animals, produce, peculiar changes in their blood and in their organs, firft connected with increafed living action; but terminating in death.

"From the experiments in this Divifion, it appears, that nitrous oxyde is rapidly abforbed by the circulating venous blood, and of courfe its condenfed oxygene and nitrogene diftributed in the blood over the whole of the fyftem.

"Concerning the changes effected in the principles of the impregnated blood during circulation and its action upon the nervous and mufcular fibre; it is ufelefs to reafon in the prefent ftate of our knowledge.

"It would be eafy to form theories referring the action of blood impregnated with nitrous oxyde, to its power of fupplying the nervous and mufcular fibre with fuch proportions of condenfed nitrogene, oxygene and light, or etherial fluid, as enabled them more rapidly to pafs through thofe changes which conftitute their life: but fuch theories would be only collections of terms derived from known phænomena, and applied by loofe analogies of language to unknown things.

"We are unacquainted with the compofition of dead organifed matter; and new inftruments of experiment and new modes of refearch muft be found, before we can afcertain even our capabilities of difcovering the laws of life."

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The laſt Reſearch, which is by far the moſt important to the practitioner, details the effects produced on the human frame, by the reſpiration of nitrous oxyde and other gaſes. For theſe we muſt, at preſent, refer to the Work itſelf.

The Doctrine of Phlogiſton eſtabliſhed, and that of the Compoſition of Water refuted: by JOSEPH PRIESTLEY, LL.D. F.R.S. &c. &c. 8vo. pp. 90, 1800. London, Johnſon.

It is inconſiſtent with our plan to enter into theoretical diſcuſſions which are not immediately connected with the healing art. Whether the truth lie on the ſide of Dr. P. or his opponents, we cannot determine; but we believe that the tide runs ſo forcibly at preſent in favour of the Antiphlogiſtic doctrines, that chemiſts had rather be carried away by it, than be at the trouble of ſtemming the current, even for the ſake of arriving at the harbour of truth.

Dr. P. examines the calcination and reduction of metals, the decompoſition of water, the conſtitution of fixed air, and azote, from all which he draws inferences in favour of the exiſtence of Phlogiſton.

A Practical Treatiſe on the Efficacy of Stizolobium, or Cowhage, (the Dolichos Pruriens of Linnæus) internally adminiſtered, in Diſeaſes occaſioned by Worms. To which are added, Obſervations on other Anthelmintics of the Weſt Indies. By WILLIAM CHAMBERLAINE, Surgeon, Fellow of the Medical Society of London, 8vo. pp. 90. London, Printed for the Author, No. 29, Aylesbury Street, Clerkenwell.

We think this practical work well worthy the attention of practitioners: It contains a ſufficient number of caſes to prove the efficacy of the remedy, as well as the ſafety with which it may be adminiſtered.

Critical Survey of the lateſt Theories on difficult Dentition in Children.

[Continued from pp. 171—177 of our laſt.]

Notwithſtanding theſe authorities, it will appear by the following obſervations, how little it deſerves to be depended upon, and how far it is from being capable of removing thoſe affections, which befall the infantile age in the period of teething.

1. It ſeems very improbable, and almoſt ridiculous to imagine, that, by this ſimple operation, the growth of the teeth ſhould be promoted in a few days, to which otherwiſe ſeveral months are required.

2. It has been ſtated, that convulſions have actually ceaſed after performing that operation; but could not its effect be thus explained, that they were removed by its having cauſed an irritation in another part of the body, and acted in a ſimilar manner as we obſerve bliſters, ſcarifications, and artificial ulcers, applied for the ſame purpoſe?

3. As it is frequently the case, that more teeth than one are preparing to penetrate, it is difficult to determine which protuberance is to be cut through.

4. The success of the operation depends in a great measure on the critical time when it should be undertaken, which is often impossible to determine. Van Swieten advises, therefore, not to perform it for fear of losing credit, as he relates a case where the tooth did not appear till two months after the operation.

5. By the experience of eminent physicians and surgeons, it has been found not only of no avail, but rather hurtful in some cases. Platner (Institutiones Chirurg. § 1078) advises not to undertake the operation, for fear of hurting the teeth. Schaeffer observes, in his translation of Armstrong, that only in a few cases it seemed to have effect, but in two others it proved unfortunate. Dessault and Gavand, (Traité d'Osteologie) confess, that it has been performed without success. Dr. Selle and Dr. Tade, two physicians of known merits, caution every body against it, because they think that the wound, by healing and causing a scar, will render the piercing of the teeth more difficult. Dr. Starke, Professor and an eminent practitioner at Jena, and Dr. Hecker, are likewise antagonists of the operation.

The application of leeches behind the ear is another remedy, whose infallibility in difficult dentition was highly praised by Le Roy, (in Esprit des Journaux, 1784, where he says of it, "C'est donc un grand moyen de population, qu'une sangsue derniere l'oreille des enfans") though it was recommended before him by Harris, (de morbis infantum); but it has been now much left off, and only kept its reputation against difficult dentition when another disease was mistaken for it and accidentally cured. The same may be said of other general remedies, which are likewise efficacious in other diseases, but which possess no particular power against difficult dentition, as blisters, &c.

Having given the arguments against difficult dentition, it may be proper to add some general reflections on the subject.

1. If any danger could be supposed to arise to children from the growth of bones, and if Nature did not prevent any morbid affections, or surmount any difficulties by proceeding always gradually in the formation of the body, dangerous symptoms might be much sooner expected from the growth of the skull, particularly when the sutures are forming, because the dura mater and the pericranium are more sensible than the soft spungy gingiva; and even the growth of the nails might more easily be supposed the cause of those symptoms, on account of the great sensibility of the neighbouring parts: but nobody ever considers them as causes of morbid infantile affections.

2. There is nothing analogous in the economy of the human body, or which had any similarity with difficult dentition, wherein by a gradual and natural growth disease is produced. The growth of other bones would as certainly excite pains and dangerous symptoms, if it was sudden or very quick; which being anomalous,

lous, is never ſuppoſed to be the caſe; and pains are not even perceived in ſome diſeaſes of the bones, in curvatures of the back-bone, or when teeth grow in a preternatural direction.

3. If the growth of teeth were able to cauſe ſuch very dangerous ſymptoms, it may be ſuppoſed, that triſmus would be particularly brought on, as it is ſo common to children; but this is only obſerved in the firſt days of life.

4. Pains ariſing from double teeth cutting through in adults, are not to be compared with dentition in children, becauſe this is preternatural and anomalous. It is with more difficulty ſuch a tooth can get through, becauſe the place which it is to occupy, is more contracted, and partly filled with the neighbouring teeth, and beſides this, the gums are not ſo ſoft and yielding as they are in a child.

5. There is no analogy between the pains ſuppoſed to be occaſioned by difficult dentition and thoſe ariſing from a caries dentium, becauſe a true inflammation ſpreading to the neighbouring parts takes place here, which originates from the nerve of the teeth being affected. Carious teeth never produce ſuch general ſymptoms, even in children, as are derived from difficult dentition; every affection remains local here, and only a little fever, or a want of appetite comes on, as is the caſe in every pain. Is it now probable, that a difference of a few years would leſſen the irritability ſo much, that, for inſtance, a child of ſix years ſhould not be attacked with convulſions from a viſible and painful affection of a tooth; whereas, it is ſuppoſed to ſuffer them at the age of one or two years from an inviſible and ſuppoſitious cauſe?

Dr. Wichmann concludes his paper by obſerving, that even if he had not ſucceeded in proving the non-exiſtence of difficult dentition, he ſhould think himſelf ſatisfied if he could but convince practitioners not to attribute too much to dentition; that this was not aſſigned as the only cauſe for all diſeaſes of children, obſervable at the time of dentition; and that it may not be neglected to point out and ſearch other cauſes, before they are derived from dentition.

Such is the opinion of Dr. Wichmann; on which we ſhall ſubjoin, (in our following Number) a few Obſervations, ſtating how far it is to be reſtrained or admitted.

Bemerkungen; i. e. Remarks on the Brunonian Practice; by D. Christ. William Hufeland, Profeſſor of Medicine at Jena, Vol. I. 1799, pp. 127, 8vo. Tubingen, Lotta.

The name of Profeſſor Hufeland has acquired ſo much celebrity, even in this country, as naturally to excite the curioſity of every medical man, to know the opinion of this great practitioner, on a ſyſtem of medicine which has of late gained ſo much ground amongſt phyſicians, and has been particularly received with applauſe on the continent. Mr. Hufeland had already communicated his judgment on it, in ſeveral numbers of his Practical Journal; but we find his ideas all united here, though, except in a few

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instances, unaltered. In *the Introduction,* he confesses himself to be, on the whole, an antagonist of this new system, in which but few ideas meet with his approbation, except such as had been received before Brown, by every reasonable practitioner. But, the chief object of this publication is, to caution young practitioners against indiscriminately applying in practice the raw principles of Brown's theory, seduced by its apparent simplicity and consistency. The manner, he proceeds, in which some enthusiasts attempted to introduce the Brunonian practice, by which they aimed at nothing less than a revolution in medicine; the spirit of faction which is thereby excited, is by no means calculated to forward science. It lies indeed, in the nature of things, that the erroneous principles and doctrines of Brown's theory will be supplanted in time; but they may leave a dangerous tendency to faction and discord, which is very prejudicial to truth and knowledge, and apt to lead to partiality, which checks the mind in inquiry. This system proceeds but from one point of view, that of incitability, and deems all auxiliary knowledge, anatomy, chemistry, &c. superfluous. *The introduction* concludes, that the Brunonian system is full of inaccuracies and sophistry, and has too many chasms to deserve the name of a system: Pure Brunonianism will produce daring physicians, not understanding how to manage Nature, but continually tyrannising over her. Against these introductory premises, may be replied by the well-wisher of that system, that, if its principles are allowed to be theoretically consistent and just, their not proving so in practice is to be derived from their being misunderstood and misapplied, which, consequently, cannot be considered as a fault of the system, but rather as a fault against it. Its imperfection can be no objection; this it shares with all systems, by which Nature is regulated. However, it excels all other, by fixing only one point of view, and avoiding a perplexing multiplicity; farther, it may be suggested, that ill consequences supposed to originate from the Brunonian method of treatment, are partly exaggerated, partly groundless, if this is built not upon the mere literal sense, but upon its true genius, a circumstance which deserves to be well weighed and distinguished. Incitability is by no means the only point of view in this system; and the extensive class of topical diseases, leaves great room for mechanic action and chemistry. The study of the auxiliary branches of medicine, is not at all rendered superfluous by it, as the celebrated *Frank,* of Vienna, has sufficiently demonstrated. How far this general apology for the Brunonian system is able to remove the above imputations, we must leave undecided, as this would necessarily lead us farther into the subject than the limits of this Journal will admit. It may suffice to remark, that many antagonists of that system, who attempted to show its errors and insufficiency, have sometimes hurt their own cause, by not having sufficiently studied and entered its genius. Whether this is the case here, we leave out readers to judge, by representing to them briefly, the contents of this publication, to which the bounds of the

the Medical Journal will only allow us to add, now and then, some remarks, which can be brought forward on the side of the Brunonian theory. The contents accordingly, are delivered under eleven different heads: 1. *Practical point of view.* In the examination of a system, it is not of so much consequence, whether it possesses a logical consistency, but whether it alleviates and improves the cure of diseases; and many systems have been forsaken, on account of this disharmony with the practice. 2. *Diagnosis of diseases* is but seemingly rendered easier by Brown's theory; for the symptoms of a sthenic state often occur in asthenic diseases, and vice versa. Brown commits, besides, the fault of considering the names of diseases sufficient to distinguish whether they are sthenic or asthenic. Moreover, the distinction of direct and indirect debility, is subject to many difficulties, and often quite impossible; for instance, in speechless persons; and sometimes such causes have preceded, that may produce as well one state as the other, which must of course perplex the Brunonian physician, or oblige him at least to state a mixed debility, in which exists at the same time, want and superabundance, a state quite contradictory in itself, which requires alike the most powerful and the weakest stimuli. Finally, it is very often impossible to find and determine the degree of incitation, and the proper remedy for it. Every thing depends on a cautious experiment; and the pretended mathematical certainty of the Brunonian theory is but a phantom. To this will be probably objected, that sthenic and asthenic symptoms are not only attended to in this system, but that, also, a chief point of it consists in determining whether a disease is topical or general; and as the present phenomena of a sthenic and asthenic state are often uncertain and fallible, it is particularly urged by this system, to look back to the preceding influences and morbific disposition for determining the diagnosis. 3. *The two species of death.* Prof. H. thinks it inconsistent with experience, that according to this system, there are but two paths to death, by direct and indirect debility: there exists a third, in the unfitness or destruction of vital organs, where neither the asthenic nor sthenic method is of any avail. However, the Brunonian will refer here to the above proximate cause, either by considering the loss of such an organ as the want of a stimulus necessary for life, or as a state primarily originating from direct or indirect debility, &c. 4. *Action of medicines.* It is a chief principle of the Brunonian theory, that the action of remedies depends solely on gradation, or the degree. However, its followers often combine indiscriminately, remedies of different degrees, viz. bark and seneka. Now, if the degree of incitement requires bark, it ought to be given alone; for seneka being a weaker stimulus, cannot add to its power, nor diminish it, as it is itself an exciting remedy: the use of such a combination is, however, proved by experience. In this manner, the Brunonian is obliged to act, inconsistent with his system, and silently to allow cases, where a specific difference of the stimuli and their chemical action take place. As instances of a mere mechanic and chemical action, the use of oil in inflammations, of alkali in gout,

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of citric acid in ſcurvy, is adduced. But the gradual difference of remedies in their action, muſt be only underſtood as far as they act upon incitability in general, but not on the individual ſtate of a ſingle organ, where different laws take place. With reſpect to the combination of different remedies, it may be ſaid, that this apparent inconſiſtency depends on our imperfect knowledge of the relative and abſolute powers of medicines, and of the preſent ſtate of incitation; and accordingly, in caſes where even a cautious experiment is not allowed, recourſe muſt be had to a combination of remedies, which experience has found to produce the neceſſary degree of incitation. The mechanic uſe of oil in inflammations is by no means denied by Brown's theory, and alkalies might be ſuppoſed not to act as ſtimuli, if they cured nothing but the gout, and if this was only cured by them. The uſe of citric acid, though ſeemingly a weakening medicine, may, in a high degree of ſcurvy, be in the ſame way explained, as the uſe of ice in frozen animals. 5. *Unequal diſtribution and action of vital power.* Under this title, Mr. H. enters into an enquiry of the ſthenic and aſthenic method, as a baſis of the Brunonian theory; and attempts to ſhow, how repugnant this is to reaſon and experience. The different parts of the body poſſeſs different degrees, and ſpecific different modifications for irritaments; as theſe act either only upon one part, or in different degrees upon all, and produce conſequently, in one, a ſthenic, in another an aſthenic ſtate. This ſeems to be confirmed by experience in nervous fevers and pulmonary conſumptions; where, in the firſt caſe, active inflammations ſometimes occur, requiring moderate bleeding and antiphlogiſtics, when the general treatment conſiſts in keeping up the nervous power by valerian, camphor, &c; in the latter, ſmall venaſections often contribute to prolong the life of the patient. May it ſuffice to ſuggeſt here, that according to Brown, both ſtates are not entirely denied to co-exiſt in different parts of the ſame organiſms, and that there is no ſpecific irritament, which does not prove for the whole conſtitution either aſthenic or ſthenic. The inſtances beſides adduced by Mr. H. are doubtful, and even not admitted by ſome Non-Brunonians. 6. *Action of cold and warmth.* Prof. H. diſcuſſes, in a dialogue between himſelf and a friend of the Brunonian ſyſtem, the queſtion, whether cold has a weakening, and warmth a ſtrengthening effect upon the body. He propoſes ſeveral arguments to prove, that the expreſſion is very relative and unſettled; that warm baths are known to render the pulſe ſlow; that inflammatory diſeaſes occur oftener in winter than in ſummer; in ſhort, that ſtrengthening and weakening are ideas too relative, to be able to aſcertain whether any thing is weakening or ſtrengthening; that cold has, beſides, an aſtringent quality, &c.; in ſhort, the contrary of this Brunonian principle may with the ſame reaſon be aſſerted. How far theſe and ſimilar arguments may appear ſatisfactory to a Brunonian, is eaſily conceived. 7. *Application of cold and warmth.* Prof. H. relates here, the caſes and circumſtances at large, in which both may be uſed as ſtimuli, as ſtrengthening and weakening remedies, according

according to what we find recorded by experience. Some of theſe ſeem indeed contradictory to Brunonian principles, or would at leaſt, with difficulty be explained after them: however, a Brunonian will not be at a loſs to extricate himſelf. 8. *Hæmorrhagies.* There are not only paſſive or aſthenic, but likewiſe active or ſthenic hæmorrhagies, originating from an increaſed action or local irritation, either idiopathic or conſenſual, which require weakening remedies, antiſpaſmodics, &c. 9. *Direct and indirect debility.* The notion of two forms of debility from increaſed or diminiſhed irritability, was entertained long before Brown received and influenced the practice; but it is erroneous to derive them, with him, from the different action of irritaments. In ſupport of this, Prof. H. brings forth ſeveral arguments; viz. that the ſame cauſes produce quite contrary ſpecies of debility; that often a viciſſitude of both ſpecies of debility occurs in the ſame individual, without the leaſt change in the external influences; in ſhort, that Brown's diſtinction of direct and indirect debility has no practical uſe. 10. *Topical and general diſeaſes.* The diviſion of diſeaſes into topical and general, already adopted by Galenus, is by no means calculated to improve the practice, becauſe no certain mode of treatment can be built upon it, as there are topical diſeaſes, the cauſes of which lie in the general conſtitution; viz. ſcorbutic ulcers, &c. as general diſeaſes are often cured by topical remedies, bliſters, purgatives, &c. The diviſion into idiopathic and conſenſual diſeaſes, ſeems therefore, by far preferable. By this it appears, that Prof. Hufeland has not quite entered into the notion which Brown has of topical diſeaſes, which is different from what has been ſtated here by him. 11. *Neceſſary retroſpect to the ſtate of the organic as well as morbid matter.* This circumſtance ſeems to be utterly neglected by Brown, by only adopting the notion of incitation and incitement. However, many diſeaſes do not depend on the degree of incitation, but on the chemical difference of organic materia, becauſe, it would be otherwiſe inconceivable, why debility could produce as well ſcurvy as gout; why alkali is hurtful in the latter, and uſeful in the former. Remedies do not therefore merely act as irritaments, but they cauſe alſo a change in materia. The whole is concluded with ſome caſes, as additional arguments againſt the faſhionable practice: Such are the objections of Prof. Hufeland. The plan of this Journal will excuſe us for not having reviewed them more at large; and we only beg leave to add, that though the above arguments ſhow the ingenuity of their author, yet they will not be deemed ſufficient by the Brunonian, to ſubvert the ſyſtem againſt which they are intended.

Apologie, &c. i. e. Apology for the Brunonian Syſtem of Medicine, grounded upon Reaſon and Experience. Edited by D. CHARLES WERNER, Phyſician in Auſtria, Vol. I. pp. 314, 8vo. Vienna, Wappler and Beck.

THE tendency of this publication is intimated upon the title, and the author informs us in the Introduction, that it is intended to defend Brown's doctrine, and confirm it by facts; a doctrine which

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he pretends to be, notwithſtanding all its excellencies, ill uſed by its antagoniſts. For this end, he invites all thoſe who believe in it, to ſupply him with communications tending to the purpoſe. In the inquiry, however, upon medical ſubjects, it ſhould be with every body a conſtant rule, not to proceed from any theory whatever, particularly if it is ſtill ſubject to doubts and objections. But this the author ſeems to have entirely neglected, by only admitting into his book, ſuch contributions which appear conſiſtent with Brunonian principles; and how little this partiality is able to promote truth and real knowledge, needs no farther demonſtration. It contains *ſome remarks on Brown's theory, a ſketch of the ſyſtem, and relations of ſeveral diſeaſes and caſes*; but on the whole, not any new idea or arguments in ſupport of it; and though it cannot be denied, that, now and then, an intereſting obſervation is met with, this cannot, however, induce us to wiſh, that the author ſhould continue a blow, for which he ſeems not altogether calculated.

Mr. Scarpa, on the Structure of the Bones.

This celebrated anatomiſt, whoſe great merits and diſcoveries in the fineſt parts of anatomical reſearches are generally acknowledged, has again given a proof of his great abilities and inquiſitive genius, by a publication that appeared laſt year at Leipſic, entitled *De Penitiori Oſſium Structura Commentarius*, 55 pages in 4to. ſplendidly printed, with three plates elegantly engraved. The common opinion hitherto prevailing amongſt anatomiſts was, that bones conſiſted of fibres, plates, or laminæ, lying in different ſtrata one upon another; but this only appears ſo in the bones of children, which, though *improperly*, may be ſaid to be formed of fibres and plates; for, on examining them through a microſcope, they are obſerved to be reticular and ramifying amongſt themſelves; it may therefore generally be aſſerted, that all bones are of a reticular and cellulous texture. To know whether the moſt ſolid, compact, and, as it were, ſtony part of a bone were cellulous, Mr. Scarpa proceeded both in an analytic and ſynthetic way. In repeating Haller's experiments on the incubation of eggs, he diſcovered by the help of a microſcope, in the thigh and ſhin bones, the fineſt reticular texture, which firſt appeared wrinkled, but on the 14th day it repreſented itſelf quite reticular, cellulous, and twiſted like cotton. This he obſerved likewiſe in the bones of an embryo of 28 lines in length. For the analytic inquiſition, he put a human bone into diluted muriatic acid. The earth being diſſolved, and the acid waſhed out, it was macerated in water, and became a woolly reticular texture. The meſhes and interſtices of it are larger in flat and broad bones than in the round ones, cloſer where the bones are compact, and looſer where they are leſs ſolid. The fineſt ſtructure has accordingly a great likeneſs to that of the ſkin. The cellular ſtructure of the bones is particularly obſervable in whales, dolphins, ſharks, and other fiſhes. The following experiments ſeem likewiſe to contradict the common opinion of the laminous

laminous ſtructure of the bones: He deſtroyed the ſhin bone of a dog, and forty days after the external rind of it was changed into a ſpongy ſubſtance of the thickneſs of 6 lines, which is the ſame caſe in mortification of bones, where nature ſurrounds the dead end of it with a thick bony ſponge, only by expanding and widening the outer cruſt of the bone. A ſimilar circumſtance happens in the *mollities oſſium* in children, where the bones receive the ſame ſpongy appearance as if they were laid in muriatic acid. A topical morbid mollification is ſometimes obſerved in bones, by which they are changed into a fleſhy maſs, very vaſcular and eaſily bleeding, which evidently ſhews an analogy with common *tela celluloſa*, forming a ſimilar fleſh in ulcers. It is very ſtriking how ſoon the bones of birds produce a ſpongy cotton-like texture when deprived of the perioſteum; "viſu mirabile eſt, in avibus quanta celeritate ex oſſibus de induſtria perioſtes nudatis, mollis caruncula propullulat; ſanguiferis vaſis plurimum referta, quæ porro in cartilaginem primum, mox intenuiſſimum quoddam oſſeum goſſypium ſubtiliter cum extus tum intus reticulatum convertitur." The bones poſſeſs a greater quantity of veſſels than is generally believed, but thoſe that penetrate into them by the *pori Haverſii* do not proceed in a ſtraight direction, but form nets in the internal [illegible]: they are likewiſe provided with nerves, though theſe cannot be anatomically demonſtrated on account of their ſubtility and cloſe ſituation to the veſſels; but it is ſhown, by experience, that the young fleſh ſhooting out of wounds of the bones is poſſeſſed of great ſenſibility. The external plate of the flat bones of the ſkull appears like diploë, when its internal lamina is already quite ſolid. The formation of cavities in bones ſeems to proceed in a mechanic way, by the hardening of the rind.

MEDICAL AND PHYSICAL INTELLIGENCE.

Prof. Abildgaard on Reſpiration.

Professor Abildgaard, of Copenhagen, has made ſeveral experiments on reſpiration, the reſults of which ſeem to be quite in oppoſition to the generally prevailing theories on this ſubject. Although they are not ſo perfect and extenſive as to ſubvert entirely the common theory, yet they deſerve the attention of every phyſiologiſt, and may contribute to a re-examination of ſeveral phænomina of reſpiration, which have been hitherto, by no means, ſatisfactorily explained. 1. It has, in general, not been ſufficiently regarded how ſmall the quantity of air is inſpired at every breath, which, according to his experiments, does not exceed three cubic inches, a portion of air only ſufficient to fill the wind-pipe (arteria aſpera). It appears, therefore, that at every reſpiration the air contained in the arteria aſpera is only renewed, [illegible]at the air in the veſicles and cells of the lungs is accordingly very ſlowly replaced by freſh air. However, a want of breath, or renewed air, only laſting a few minutes, is able to kill animals, which does not ſeem to correſpond with this phenomenon. 2. Experience ſhews that the retention of breath cauſes the ſame uneaſineſs and inconveniencies after inſpiration as after expiration; though, according to theory, it ſhould be eaſier to retain the breath after inſpiration. 3. The wind-pipe was opened in two dogs, and by a convenient apparatus, azotic gas injected into the lungs of one, and oxygen gas into thoſe of the other; and after the lungs ſeemed thereby perfectly extended, a ligature was applied to their wind-pipes. Both dogs felt the ſame painful ſenſation in the attempt of gaſping for breath, and there was no difference perceived except that the latter lived eleven minutes, whereas the former died in five. The movement of the heart ſtill remained for ſome time viſible in both after the breathing had ceaſed. The queſtion now is, why both animals attempted to breathe with equal ſenſation of pain and uneaſineſs, though one had the moſt ſalubrious air in the lungs? and why did they die before the movement of the heart, and conſequently the circulation had ceaſed? 4. It has been obſerved by repeated experiments, that animals being in a ſtate of aſphyxia, and brought to life again by air being injected into their lungs, did not catch breath before the heart began to beat; if, therefore, the motion of the heart is ſuppoſed to ceaſe from want of oxygen, why does it beat in this caſe, before breathing is re-eſtabliſhed? 5. Air drawn from the lungs of a dog killed by a ligature of the wind-pipe, was found to contain a portion of oxygen, and yet the dog died for want of breath, which ſeems

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contrary to the common opinion of the uſe of oxygen, &c. Prof. Plaff of Kiel, remarks, in oppoſition to theſe experiments, that No. 1, is inconſiſtent with other experiments made on this ſubject by Robert Menzies (in his Tentamen Phyſiologicum de Reſpiratione) with the greateſt exactneſs, as it ſeems, according to him, forty-three cubic inches are always inſpired at once. They do not likewiſe correſpond with the experiments of Lavoiſier, Abernethy, and Goodwyn, as, according to the latter, about ten cubic inches of vital air, (oxygen gas,) and nearly forty cubic inches of atmoſpheric air, are breathed in at every reſpiration. The difference between theſe experiments and Prof. Abildgaard's is conſequently too great, and ſome inaccuracies muſt have been committed on one ſide or the other. It is therefore to be wiſhed, that Prof. A. would relate his experiments at large, to enable us to judge to which of them credit is to be given. The obſervation in No. 3, is indeed intereſting, but could not the ſame painful gaſping for breath in the dog, whoſe lungs were filled with oxygen gas, be derived from the preternatural extenſion of them, and the mechanical impediment cauſed thereby in the circulation of the blood? the ligature itſelf may have had ſome influence here. Beſides, it is probable that the carbonic acid evolving itſelf in the lungs extended by oxygen gas, may have produced the ſame phenomena in this dog as the mephitic air in the other, particularly as we know that a mixture of air, in which oxygen gas is preſent, may become fatal by an additional quantity of carbonic acid; and this likewiſe explains why the animal (No. 5) died, though the air in its lungs contained a portion of oxygen gas. The previous motion of the heart (No. 4) before reſpiration was perceived, may, notwithſtanding, be owing to the oxygen entering the lungs with the injected air, which ſtimulated the heart previous to the re-eſtabliſhment of reſpiration. *(See Northern Archives of Medical and Natural Science, by Prof. Plaff and Dr. Scheel, Vol. I. No. 1. in German.)*

Method of obtaining Sugar and Brandy from the Beet Root.

The committee for examining Mr. Achard's mode of fabricating ſugar, conſiſting of the moſt reſpectable chemiſts at Berlin, has now, by order of the King of Pruſſia, publiſhed an inſtruction, containing the method of making that indigenous ſugar, of which the following is the moſt eſſential:—The beet roots beſt calculated for the extraction of ſugar, are thoſe which have ſoft fleſh, whitiſh towards the edges, and not growing above ground. After being cleaned, they are boiled, cut into pieces, and pounded in a wooden trough with wooden ſtampers, and afterwards preſſed. The juice thus obtained is immediately put into a poliſhed copper kettle and ſimmered, during which time the ſcum appearing on its ſurface muſt continually be taken off. To one hundred quarts of this juice, two ounces or leſs of ſlacked lime, diluted ſo as to have the appearance of milk, are added, and the boiling continued till the juice is thickened to the half of it. Having ſtrained it through

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a woollen cloth, it is thickened in a polished copper vessel, or, which is better, in a tin vessel, to the consistency of a syrup, which afterwards is put into glass, stone, or wooden vessels. These being placed near a moderate fire, saccharine crystals appear, which being freed by expression from the mucilaginous juice, a very good raw sugar is obtained, which must now be left to the refiner.

For the preparation of brandy, the water used in the first boiling of the roots, is boiled again, and poured on the residuum from the first expression of the pounded roots; this must stand for a day or two, after which it is expressed, and the remaining dry pulp serves as a good food for cattle. The juice obtained in this way is mixed with the waste parts of the syrup and the mucilage which remains after the expression of the saccharine crystals, and all boiled together till half of it is evaporated. The liquor is then poured into a coop exposed to a temperature of 45° Fahrenheit, and cooled to 65°. Having added a proportionate quantity of yeast, it is left to ferment, and in three or four days after the distillation is undertaken. The taste and smell of turnips are removed by the addition of well-burned pulverised charcoal; but the brandy thus obtained has a great deal of the flavour of rum. According to the experiments made by the Committee, 15 quintals of beet-root afforded $57\frac{1}{2}$ lb. of raw sugar and near five gallons of brandy.

We have authority to announce the following Medical Lectures.

ST. THOMAS'S AND GUY'S HOSPITALS.

The Lectures for the ensuing Season will commence at these united Hospitals in the following order:—

Theatre, St. Thomas's Hospital.

Anatomy and Surgery, on Wednesday, October 1, at one o'clock, by Mr. CLINE and Mr. ASTLEY COOPER.

Theatre, Guy's Hospital.

Theory and Practice of Medicine, on Friday, October 3, at ten in the morning, by Dr. BABINGTON.—Midwifery, and Diseases of Women and Children, on Saturday, October 4, at eight in the morning, by Dr. LOWDER and Dr. HAIGHTON.—Chemistry and Experimental Philosophy, on the same morning at ten, by Dr. BABINGTON and the Rev. Mr. ROBERTS.—Physiology, or Laws of the Animal Economy, on Monday, October 6, at a quarter before seven in the evening, by Dr. HAIGHTON.—Therapeutics and Materia Medica, on Thursday, October 9, at seven in the evening, by Dr. CURRY.—Principles and Practice of Surgery, on Monday, October 13, at eight in the evening, by Mr. ASTLEY COOPER.——Mr. COOPER will also deliver a Course of Lectures on Select Surgical Cases in the Hospital.

In addition to these, early in October, a Course of CLINICAL LECTURES will be begun by Dr. SAUNDERS and Dr. BABINGTON, and continued through the Season on the following plan:

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The most interesting Cases will be selected from among the whole number of Medical Patients admitted into the Hospital.

As soon as possible after admission, a History of the Case will be taken down at length; in which will be described the Patient's age, temperament, occupation, and condition in life; the date and progress of the complaint; the actual or probable causes giving rise to it; the medicines, if any, already employed, and the effects they have had; together with the symptoms present, whether collected from the Patient's feelings, or cognizable by the Physician. There will also be given a daily Report, specifying the remedies prescribed, their sensible effects, and the changes which take place in the disease. Every Thursday evening, at eight o'clock, a Lecture will be delivered upon the most important cases; in which the symptoms of each will be examined, the leading and essential ones pointed out, and the disease referred to its proper place in the best Systems of Nosology. A comprehensive view will be taken of the plan of cure to be followed in such cases; the several indications will be laid down; and the particular circumstances, either originally belonging to the case, or arising during its progress, and which may render a deviation from the general plan necessary, will be specified. The ordinary and expected effects of the remedies prescribed will be mentioned; their operation distinguished from the consequences of the complaint; and any new or unusual result from them will be carefully noted, and, as far may be, accounted for. The symptoms that portend a favourable event, or the contrary, will be marked as they occur, and connected with the previous circumstances, in order to ascertain the dependance of the one upon the other, and afford data upon which to ground an early and correct prognostic. Lastly, where the termination is fatal, and the case is such as to be elucidated by examination of the body, this will, if possible, be effected, and a full account given of the morbid appearances. In short, throughout the whole Course, the great object will be to unite Theory with Practice, to verify general precepts by individual examples, and thereby to give a connected and systematic view of the History, Nature, Causes, and Cure of Disease.

CONDITIONS.

1. Clinical Pupils to pay five guineas for the Course, which will commence in October, and end in May; the first four months to be conducted by Dr. Saunders, the next by Dr. Babington.

2. No person to be accepted as a Clinical Pupil, who is not intitled to attend the Lectures on the Practice of Medicine during the time the Clinical Course will continue.

3. Clinical Pupils to have the privilege of going into the Clinical Wards only, unless they are likewise Pupils either to the Physicians or Surgeons of the Hospital; and then only during Hospital hours.

4. Gentlemen entering to the Physicians as perpetual Pupils, or for twelve months, to have the benefit of the Clinical Lecture without additional expence.

TERMS

Terms of attending as Physicians Pupils.

For six months, 10 guineas.—For twelve months, 15 guineas.—Perpetual, 21 guineas.

The following Lectures will commence at the London Hospital on the 1st of October: On the Theory and Practice of Physic, by Dr. Cooke; on Chemistry, by Drs. Hamilton and Frampton. The Clinical Lectures, by Drs. Cooke, Hamilton, and Frampton. The Anatomy, Physiology, and the Principles and Operations of Surgery, by Mr. Blizard and Mr. T. Blizard. The Anatomical Demonstrations by Mr. Headington.

Theatre of Anatomy, Great Windmill Street.

Mr. Wilson will commence his Course of Lectures on Anatomy, Physiology, Pathology, and Surgery, on Wednesday, the 1st of October, at two o'clock.

A Room is likewise open for Dissections, under the direction of Mr. Wilson and Mr. Thomas, from nine o'clock in the morning till two in the afternoon, from the 10th day of October till the 20th of April; where regular and full demonstrations of the parts dissected are given; where the different cases in Surgery are explained, the methods of operating shewn on the dead bod[illegible] [illegible] where also the various arts of injecting and making Preparations are taught.

Dr. Fordyce will commence his Au[illegible] Course of Lectures on the Practice of Physic, Materia Medica, and Chemistry, at his house in Essex-street, Strand, on Monday the 6th of October, at eight o'clock in the morning.

Dr. Pearson's Lectures on Physic and Chemistry will commence the first week in October next, at the Laboratory in Whitcomb-street, Leicester-square. The Lectures on the Materia Medica and Practice of Physic are delivered every morning, from a quarter before eight to a quarter after nine, and on Chemistry from a quarter after nine to ten. Lectures on the Cases of Hospital Patients are also given once a week.

Dr. Crichton will commence his usual Autumnal Course of Lec[illegible] [illegible]eory and Practice of Physic, Chemistry, and Materia Medica, on Monday, October 13, at his house in Clifford-street, Bond-street, at eight and nine o'clock in the morning.

Dr. Bradley will commence his Autumnal Course of Lectures on the Theory and Practice of Medicine, on Monday, October 6, at the Lecture Room, No. 102, Leadenhall-street, at six o'clock in the afternoon.

Dr. Osborn's and Dr. Clarke's Lectures on Midwifery and the Diseases of Women and Children will be given the following winter,

winter, as usual, only at Dr. Clarke's house, No. 1, in New Burlington-street. The Course will begin on the first Monday in October next, at half past ten o'clock.

Dr. DENNISON and Dr. SQUIRE, on Wednesday, Oct. 1, will begin a Course of Lectures on the Principles and Practice of Midwifery, and the Diseases of Women and Children. In addition to a constant supply of Labours, Gentlemen may have frequent opportunities of visiting patients in disorders during pregnancy, and after delivery.

Dr. BATTY will begin his Course of Lectures on the Theory and Practice of Midwifery, and on the Diseases of Women and Children, on Monday, October 6, at his house, No. 6, in Great Marlborough-street, at eleven o'clock in the morning.

On the ninth day of the ensuing month, October, T. POLE, Man-Midwife to the Obstetric Charity for the Delivery of poor Women at their own Habitations, will commence his Autumnal Course of Lectures on the Theory and Practice of Midwifery, including the Diseases of Women and Children, at his house, No. 102, Leadenhall-street, near the Royal Exchange, London.

Mr. PEARSON will commence his Autumnal Course of Lectures on Surgery, at his house in Golden Square, on Monday, Oct. 6, at seven o'clock in the evening.

Mr. CHEVALIER'S Autumnal Course of Lectures on the Principles and Operations of Surgery, will commence on Monday, October 6, at his house, No. 20, South Audley Street. The terms of attendance are Three Guineas.

Mr. CARPUE will continue his private Anatomical Lectures, at his house, No. 44, Leicester-square. Particulars may be known by applying to Mr. C.

Messrs. A. and C. R. AIKIN will resume their course of Lectures on Chemistry and the Chemical Arts, early in the winter, at the house of Mr. C. R. Aikin, Surgeon, Broad Street Buildings.

In the press, an Essay on Phlegmatia Dolens, or the painful Swelling of the lower Extremities incident to Lying-in Women. In which a literary history of this disease, comprehending a review of the different theories that have been formed concerning it, from the earliest records of medicine to the present time, will be given; and its analogy with peritonitis puerperalis, or child-bed fever, will be pointed out and elucidated by a comparison of their symptoms, terminations, predisposing, remote and proximate causes, and their cure. By JOHN HULL, M.D.

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The ſame Author has prepared for the preſs, an Epitome of the Noſologia Methodica of Sauvages, which is intended to be publiſhed by ſubſcription. The propoſals and a proſpectus of the work will appear in a few weeks.

A ſecond edition is in the Preſs, and will be ſpeedily publiſhed, of Dr. CHISHOLM'S Eſſay on the Malignant and Peſtilential Fever of Grenada, greatly enlarged; comprehending additional facts and illuſtrations in ſupport of its nature and treatment, as formerly deſcribed; with obſervations on contagion, the endemial fevers of the Weſt Indies, the effects of the nitrous acid and oxygenated muriate of pot-aſh in ſeveral diſeaſes, &c.

NEW MEDICAL PUBLICATIONS IN AUGUST.

New Inventions and new Directions to the Ruptured, by a private Gentleman. Low.

A Treatiſe on the Chemical Hiſtory and Medical Powers of ſome of the moſt celebrated Mineral Waters, with Practical Remarks on the Aqueous Regimen. To which are added, Obſervations on the Uſe of Cold and Warm Bathing, by William Saunders, M. D. 8s. boards. Phillips, George Yard.

A Practical Treatiſe on the different Fevers of the Weſt Indies, and their diagnoſtic Symptoms, by William Fowle, M. D. 2s. 6d. H. D. Symonds.

NEW MEDICAL PUBLICATIONS IN GERMANY.

Ueber die Pulſader Jeſhwulſte, i. e. On Aneurisms and their Surgical Treatment; by Dr. AYRER, with a plate, pp. 387, 8vo. Gottingen, 1800, Dieterich.

Beitrag zur Beurtheilung, i. e. Contribution to the Criticiſm of the Brunonian Syſtem of Medicine; by LEWIS CHRIST. WILLIAM CAPPEL, Doctor of Medicine at Gottingen, ſecond edition, much altered, pp. 448, 8vo. 1800. Gottingen, Dieterich.

Ritter's Darſtellung, i. e. RITTER'S Hiſtory of the lateſt Inquiries relating to the Illumination of Phoſphorus in Azote, and of the Reſults of it for Chemical Theory, 8vo. 1800. Jena, Fromman.

G. J. Beer's Methode, i. e. Method of extracting the Cataract along with the Capſula; with ſome other improvement of the operations of the Cataract in general. 1799, pp. 60, 8vo. with a plate. Vienna, Schaumburg.

To CORRESPONDENTS.

Our Correspondents are particularly requested to give the TITLES *of their papers as well as the* Title of the general Subject.

We have received friendly and wholesome advice, which we hope to profit by, from an anonymous Correspondent respecting controversial papers, and those of young authors; but we should have been more obliged to him if he had given us his address, as we might then have availed ourselves of the advantage of his opinion on other occasions.

Communications have been received from Meſſrs. Malin, Favell, Clutterbuck, Ballard, Thackeray, Grose, Bellamy, and S. S. Doctors Carson, Gillespe, and Simons.

Mr. Waiblinger's Bandage for a dislocated Patella.